CORRESPONDENCE

CONCERNING THE

GREAT MELBOURNE TELESCOPE.

IN THREE PARTS:

1852—1870.

Printed, for private circulation only, by order of the Council of the Royal Society.

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PART I.

1852–1853.

[*For private circulation only.*]

THE ROYAL SOCIETY.

CORRESPONDENCE

OF

THE SOUTHERN TELESCOPE COMMITTEE.

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THE SOUTHERN TELESCOPE COMMITTEE.

At a Council of the Royal Society (November 25, 1852),—

The following Resolution of the Council of the British Association was read :—

"That it is expedient to proceed without delay with the establishment in the Southern Hemisphere of a Telescope not inferior in power to a three-feet reflector; and that the President of the British Association, with the assistance of the following gentlemen, viz. The Earl of Rosse, Dr. Robinson, Lord Wrottesley, J. C. Adams, Esq., The Astronomer Royal, J. Nasmyth, Esq., W. Lassell, Esq., Sir D. Brewster, and E. J. Cooper, Esq., be requested to take such steps as they shall deem most desirable to carry out the preceding resolution."

Whereupon it was Resolved,—That the President and Council agree with the British Association in considering it desirable to proceed without delay in obtaining the establishment of a telescope of very great optical power for the observation of Nebulæ in a convenient locality in the southern hemisphere, and that a Committee be appointed to take such steps as they deem most desirable to carry out this resolution. The Committee to consist of the President, Officers, and Council of the Royal Society, with the addition of the following gentlemen: Lord Wrottesley, Dr. Robinson, J. C. Adams, Esq., The Astronomer Royal, J. Nasmyth, Esq, W. Lassell, Esq., Sir David Brewster, E. J. Cooper, Esq., Sir John Herschel, Sir John Lubbock, and the Dean of Ely*.

The following Correspondence which has taken place amongst the Members of the Committee then appointed, is printed by order of the President of the Royal Society.

* The name of John Phillips, Esq. was added at a subsequent Meeting of the Council.

CORRESPONDENCE.

Mr. Nasmyth to the Earl of Rosse.

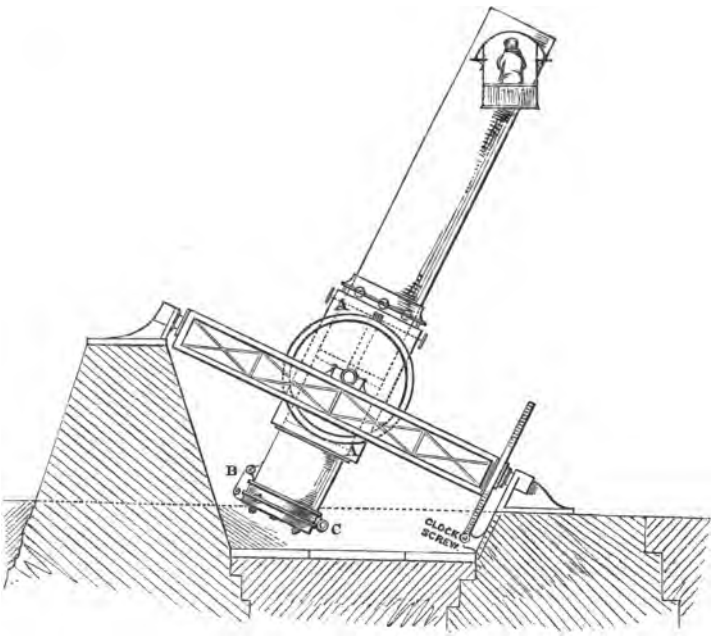
Bridgewater Foundry, Patricroft,
near Manchester, Dec. 15, 1852.

My Lord,

With respect to the system of mounting such an instrument as is proposed, I should vastly prefer the equatorial, and do not think that even in respect to expense it would prove so much more costly than the altitude and azimuth, and decidedly superior as to general steadiness and ease of management, and for securing by clock-work movement perfect tranquillity to the observer, who I would propose to place in a very snug box or box-seat slung to the eye-piece socket. After selecting his object he would have nothing to do but sit at his work, carried along with the instrument, and the object ever in the field. As the powers requisite to use up all the light of a four-foot metal must be seldom much under 360 to 400, were we to attempt to follow the object by other than equatorial clock motion, I fear there would be little of that tranquillity so requisite in making careful observations and sketches with so powerful an instrument.

The following rude sketch is somewhat like the way in which it appears to me such an instrument should be mounted. The polar axis is a strong frame of cast iron, between the sides of which the telescope-tube swings; the observer is in his snug box, slung to the eye-piece socket, so as to sit always on a horizontal seat whatever may be the position of the instrument, the seat of his box being universal for that object; the tube to revolve in the square socket bore A, so as to give means of correcting "the screw" of the tube consequent on the equatorial arrangement, and in that way keep eye-piece, observer and metal always in one constant or nearly constant parallel. I would recommend my? plan of having the metal in a hinged cast-iron cell, so that by a small windlass at B you can let back the cell

on the hinge C, and get in to wipe it, or cover it with the utmost care. I find this a most handy system.



With sufficient mass of material in the frame or polar axis, I should not fear any want of steadiness. Even in such a windy night as would be at all fitted for observation, the observer might close his shutters and be as snug as at his own parlour fireside; for when once he had sat down to his work on the object then in hand, with the clockwork in gear, he need not trouble his attendant for the next three hours, if he select such objects as are near the meridian. With respect to the cost of such an instrument, I have made a rough estimate of what appears to me would be a fair price for so out-of-the-way and anxious a job, including a suitable polishing machine, which I conceive to be absolutely requisite. I should say the maker, whoever he may be, ought to have £3000 at the least. I include in this two metals of not less than 6 inches thick, ground on the back to the same curve as the face, supported of course on your Lordship's admirable plan of, say, 81 self-adjusting points or friction balls. I would fain have had the focal length equal to 9, or at least 8, diameters of the metal.

If we could prevail upon Messrs. Maudsley to undertake this work, I think we could supply them with all the information as to casting, grinding and polishing which they would stand in need of.

(Signed) JAMES NASMYTH.

To the Right Honourable
The Earl of Rosse, &c. &c.

The Earl of Rosse to Colonel Sabine.

Castle, Parsonstown, December 18th, 1852.

DEAR SABINE,

The equatorial mounting as proposed by Nasmyth would be decidedly preferable to the altitude and azimuth mounting of our three-feet telescope, provided it was steady. The worst consequence to be apprehended, should there be a want of steadiness, is *impaired definition*. Of course with an unsteady instrument, micrometer-work would be very unsatisfactory, but that *comparatively* is a subordinate consideration. Nasmyth's mounting is in principle what has been called the *English mounting*, a long polar axis being employed instead of a short bearing. So far as I am aware, no large instrument so constructed is steady except the Liverpool equatorial. In the case of the Liverpool equatorial the weight of the telescope bears a very small proportion to the weight of the polar axis, and that, in my opinion, is the reason why it is steady. I am speaking merely by guess, but I should think the weight of the telescope is probably not more than $\frac{1}{16}$th the weight of the polar axis. On this point, and I think it a very important one, precise information could easily be obtained. The equatorial here is on the English plan, and is, I believe, the largest instrument of the kind. It is not perfectly steady, and I am sure it would not work satisfactorily in the open air. The speculum is 18 inches aperture, and 10 feet focus; the polar axis is 14 feet long, and $3\frac{1}{2}$ feet largest diameter, made of wood like a cask. The staves are very strong, and were impregnated with sulphate of copper; then baked, and closely hooped. After hooping, the whole was baked, and the hoops re-driven. The ends are of oak, in alternate layers 12 inches thick, glued and bolted together, to which the staves are attached by thirty dozen of screws, aided by glue. The whole was covered with a cement of bees'-wax resin, whiting, and spirit of turpentine to protect it from hygrometric action. The tube of the declination axis is contained in a copper pipe, which makes air-tight joints with the polar axis, so that damp air is excluded. The polar axis was turned to receive its fittings, and so well has it kept its figure that it now runs as true in its bearings as it ever did. Its stiffness is so great, that a small telescope, magnifying about fifteen times, having been fixed to one extremity, and looking at a fine mark fixed at the other extremity, (so as to test flexure) when the polar axis with its full load was turned round 180°, the flexure was imperceptible. With this great strength, however, the torsion is sufficient to produce considerable unsteadiness in right ascension in handling the instrument. The declination axis, on the other hand, is but $3\frac{1}{2}$ inches thick at the bearing close to the telescope, and yet no instrument can be freer from tremor in declination. In the one case the telescope is 7 feet from each bearing, and in spite of enormous strength we have torsion and tremor; in the other case it is scarcely 3 inches, and there is no perceptible tremor or torsion, though the axis is comparatively feeble. The weight of the telescope may be to the weight of the polar axis as about 1 to 4. In Mr.

Nasmyth's mounting the telescope can scarcely be less than 12 feet from each bearing; and in my opinion nothing but *enormous mass* can prevent torsion, and so much unsteadiness, as will render the instrument of little value in the open air. It will be necessary to form at least some rude conception of what the mass should be; unfortunately in these motions we cannot safely trust to mere dynamical considerations; there is much which is very obscure, and we must, as it were, feel our way. As the size of instrument increases, the proportionate strength of materials diminishes; it would not therefore be safe to infer that a polar axis sixteen times heavier than the telescope would be sufficient. There may, however, be an excess of strength in the Liverpool polar axis, and probably this question might be tested by loading the telescope, if the telescope is strong enough to bear this rough treatment without injury. Suppose the same proportions as the Liverpool equatorial should be considered safe, which I have assumed to be as 1:16: roughly speaking, the weight of the telescope may be somewhat as follows:—

	Tons.
Speculum.....	1½
Box and levers	½
Tube..... ..	4
	6
	16
	96 tons, weight of polar axis.

This at £20 per ton, including patterns and millwright's work, would be £1920 as the cost of the polar axis. The brass circle and clock would, I should think, be at least £200 or £300 more; the tube and fittings perhaps £500, and the two specula £1000. The polishing machinery with a one-horse steam-engine to drive it, would cost perhaps £300.

£1920
300
500
1000
300
£4020

There should be an unusually large margin for contingences, so that, I think, it would be scarcely safe to estimate the cost at less than £6000.

Were it determined to execute such an instrument, it would be highly desirable that a good working model, furnished with an excellent telescope, should be made previously, and thoroughly tested. The telescope itself should also, in my opinion, be put up near the maker's in a temporary way, and well-tried at *real work* before it was sent out. In the event of failure the tube would answer for an

altitude and azimuth mounting. An instrument mounted like my three-feet telescope would cost, I think, about one-half. The equatorial mounting, if really successful, is so much better than my plain stand, that if the Government could be prevailed upon to incur the expense the experiment certainly ought to be tried. I have just received Mr. Smyth's ingenious plan for mounting a large telescope equatorially, but have not considered it with the attention it deserves. I fear the cost would be enormous, and I scarcely think the mass could be safely made materially less than in Nasmyth's. The distance between the pivots of the polar axis, and the point to which the driving force would be applied, is so considerable, that there would be a strain which the form would not be well calculated to resist; and where there is much torsion I fear it would be difficult to control the tremors to which it gives rise. Should the Messrs. Maudsley be disposed to undertake the instrument, Mr. De la Rue could I am sure assist them in the polishing, and when in London I should, of course, be very happy to afford them any information in my power. Extreme pressure of business of different kinds has prevented me from writing to you as soon as I intended. I should have been glad to try the experiment, whether the movements of the observer, placed as proposed by Mr. Nasmyth, would be likely to disturb the instrument, but the weather has rendered it impossible.

(Signed) Rosse.

Postscript.

I omitted to add to my letter of yesterday that I think the German mounting is preferable to Nasmyth's, and to every other which has been suggested. It has its disadvantages, its small circles being objectionable; still, looking to the two great questions, steadiness and expense, I think it has greatly the advantage. I think it would be safer to speak with confidence about it than any other. I have not seen Mr. Cooper's, but I believe from what I have heard it is perfectly steady, and the telescope is unusually heavy for its length and aperture. It is used in the open air, with however some shelter from a wall. Mr. Cooper's is the German mounting, and I think it might in some measure be taken as a large working model.

Very truly, &c.,

Rosse.

Sir D. Brewster to Mr. Weld.

St. Leonard's College, St. Andrews, Dec. 20th, 1852.

Dear Sir,

I may mention to you, what I thought it right to state to Lord Wrottesley, that when I was on a visit to Lord John Russell last autumn, I was describing to him Lord Rosse's great telescope: he, of his own accord, said, "Would it not be a right thing to send a large telescope to a southern climate?" I replied, "Certainly; but your Lordship would not give us money to do it;" and I added, "that I felt the disappointment particularly, as I was President of

the British Association when it was refused." Lord John said that he had no recollection of the matter; and I stated in a way not to be misunderstood, what I firmly believe is the truth, that some interested person had interposed on the occasion and prejudiced the Government against the scheme.

If another application is made to the Government, which, of course, it will be, I trust that the Committee will take care that it is made by persons *whose hearts are really in the cause*, and that they will take care to meet any underhand opposition that may be made to it.

I consider Prof. Smyth as pre-eminently qualified to take charge of the new establishment, and I look forward to results of the highest kind from his superintendence.

I am, dear Sir,

Ever most truly yours,

C. R. Weld, Esq.,
Assist. Sec.

D. Brewster.

Mr. C. P. Smyth to Capt. Smyth.

London, December 22, 1852.

My dear Father,

Finding that there is a greater length of time to spare than had been expected before the next Meeting of the Southern Telescope Committee, I take advantage of the opportunity to write more fully on several of the topics which are to be considered. It may lead to a simplification of the question if I withdraw from the conditions mentioned in your letter of December 7th to Mr. Bell the passage to the effect, "that the new establishment was not to be inferior to that of the Cape." There then only remain,—

1. The mounting of the Telescope.
2. The site of the Observatory.
3. The publication of the results.

The mounting of the Telescope.—The telescope is, I believe, to be a Newtonian reflector, with an aperture of 4 and a focal length of 30 feet. Its weight therefore will be very great, and a mounting of the altitude and azimuth construction would be the simplest mechanical solution of the problem of enabling it to be pointed to every part of the sky.

But in the present case, where not only eye-views of the nebulæ are required, but accurate numerical measures of them also are indispensable, and should form the leading feature of the establishment, the simple altitude and azimuth-stand will not be found sufficient, without at least the assistance of some additional directive apparatus for equatorial motion, such as that devised by Mr. Airy for some temporary stands at Greenwich.

Were there however sufficient truth of movement obtained by these means, and in a sufficiently simple manner for the purpose of micrometrical measures, it would be found still more difficult to keep up the motion with that perfect uniformity through a long period of time, which would be absolutely necessary in the applica-

tion of photographical processes to picturing the field of view. This is a method too which it is of the highest importance to introduce, and which, though at present applicable to but a limited range of objects, will without doubt be continually undergoing important extension.

The very strict requirements of this latter mode of procuring results would seem to oblige us to adopt the parallactic form of mounting with clock-work motion. In principle nothing can be better; and in practice it has proved, in various instances, all that could be wished. Still it is apprehended that there would be great difficulty in applying it to so heavy an instrument as a 4-foot speculum.

On looking around we certainly find that no equatorial mounting strong enough for such a reflector has yet appeared, but that is no proof that it is impossible. Often by discovering errors of principle in the older constructions, or by employing stiffer materials and greater mass, has the principle been found capable of extension to larger sizes. And the recent immense advances in the manufacture and the employment of both cast and wrought iron, may be taken as an assurance that a far more powerful parallactic instrument may now be produced than has ever yet been seen; and I will enclose a plan of my own, by which I have strong hopes that a mounting of almost any size and strength could be formed.

The site for the Observatory.—In addition to all the facilities for producing good observations, afforded by the space-penetrating power of a telescope, as well as by the style of its mounting, much must always depend on the latitude of the station selected, and more still on its elevation above the level of the sea.

This arises mainly from the disturbing effects, always present in a greater or less degree, of the atmosphere, increasing too with the aperture of the telescope, as well as with the magnifying power employed. But by simply mounting up 1000 feet above the level of the sea, the quantity of the atmosphere to be looked through, in a zenith observation, will be diminished by so large a portion as $\frac{1}{30}$th of the whole.

Thus there were easily observed with Bradley's zenith sector on the Cape Mountains (2000–5000 feet high) certain small stars, of which nothing could afterwards be made at the Cape Observatory; while with larger stars, seen at both stations, the mountain-observations were more accordant, and in so far better than the others, owing apparently to the momentary fluctuations of refraction decreasing in proportion to the rarity of the air.

Elevation is also of importance in raising the observer above the *mechanical* impurities of the atmosphere, such as fine dust, smoke, and the vaporized particles of solid bodies, well known under the familiar term of motes in the sunbeam. These, rather than the gaseous constituents, are what produce the general diffusion of light over the sky impeding photographic action, and, if abundant (according to the experience of a friend in South Africa) preventing the attainment of the highest polish on specula.

If completely immersed in this dust, we are not conscious of the

extent of its effect; but on rising up the side of a mountain, even on reputedly clear days, we then plainly see the impure medium below. All objects within its reach are then observed to have their edges indistinct and thin, the dark and bright parts confused together; while the sharpness of the outlines, the pungency of the light, and the transparency of the shadows of anything above its range, form a most striking contrast.

When a star by night, or a heliotrope by day, was seen through this dusty matter at the Cape, it was invariably blurred into a diffuse sort of nebula, and the space-penetrating power of its light was greatly diminished, while nebulæ proper were rendered invisible. And if the medium was of more than ordinary density, it was found to prevent even the largest heliotropes being seen at short distances, though their light would otherwise penetrate with facility through far darker vapour when of the watery kind, and through actual rain, neither of which seem to have any prejudicial effect on definition; indeed, the only good observing nights at the Cape were when dew was falling.

From the very general distribution of this finely divided matter, the lowest stratum of the atmosphere, for a thickness of about 5000 feet on the average, may be regarded as a dry and dust-bearing region; above which commences a moist and cloud-bearing one, and rises to a greater height than has yet been reached by man. From the result of two years' experience, on the S. African mountains, both a daily and an annual variation in quantity are manifested, the dust being more abundant in the afternoon than in the forenoon, and in the autumn than in the spring; or in much the same ratio in which photographers experience a retardation of the chemical action of the solar rays, owing very probably to the yellowish colour transmitted by the particles.

They appear moreover to be confined as to their range of altitude in the summer to 3000 feet, while in the winter they often rise above 7000 feet—an effect, Professor Stokes has suggested, of the *viscidity* of the gases composing the atmosphere and bearing the particles of matter, combined with the effect of heat on that quality.

From this partial sinking of the dust in summer there results a concentration of it near the surface of the earth, in proportion to the warmth of the country and the season. Hence perhaps the remarkable fact that the chemical action of the sun's rays is much weaker there, notwithstanding the superabundance of light, than it is in the temperate and less illumined zone. This circumstance, while it increases our difficulties at the level of the sea, relieves us of them more and more completely on elevated situations, in proportion as the heat is greater, which, conjoined to the more bearable temperature for the observer, points out the advantage of employing an *elevated* situation in the *tropics* for the observation of nebulæ.

Of the three plans already mentioned at the Committee, viz.

Cape Town in lat. S.	34°
Sidney	34
Hobartown	42

none suit the above conditions very well, either as regards latitude or elevation, for they are none of them sensibly elevated above the level of the sea, and it would be necessary at least to abandon the towns and seek more appropriate sites in the adjoining country.

But even with this modification nothing very favourable can be elicited; for at the Cape the best position afforded is the Khamisberg, with an inhabited summit 5000 feet high. But it is almost inaccessibly beyond broad, difficult tracts of desert land, and is almost entirely deficient of building-materials and labourers. Australia is put out of the competition by the effects of the gold discovery; and in Van Diemen's Land, though some circumstances are more favourable, as the abundance of wood, of convict labour, and of good public roads, yet the highest available table-land there seems to be not much more than 2000 feet high.

If therefore the utmost advantage is to be taken of natural circumstances to second the optical power of the telescope, recourse must be had to other countries, and of all in the Southern hemisphere, the elevated plains of the Andes would appear to be the most favourable, and might be adopted but for political difficulties. These, however, would fortunately offer no bar to employing the new Sanatorium of Ceylon, where the height of at least 6000 feet could be gained.

Something might thus be lost on the three first stations as regards latitude for Southern objects, but much would be gained in altitude; and if photography is to be applied to the registration of telescopic phenomena, every other consideration should give way before this most important one of elevation of site, and for this reason.

The atmosphere is never in so perfect a state, that a highly magnified image in a telescope appears perfectly quiescent; it is always fluctuating more or less with a variety of motions and alterations of shape. In ordinary observing the eye follows all these movements, and is able to acquire the same exact idea of the star as it may of the masts and rigging of a vessel that is slowly heaving at sea, though such motion during the formation of a camera-photograph would prevent anything appearing beyond a blurred mass in the mean position of the hull for the time. As many of the optical tremors too of the atmosphere are so rapid as to be repeated many times in a second, no improvement in photographical processes, short of perfect instantaneity, can remove this serious practical difficulty. And the only mode by which we can at least diminish it, is by decreasing, through elevation of site, the quantity of the atmosphere through which we look.

The publication of the results.—The establishment of any observatory by Government being for the benefit of science at large, the speedy publication of the observations at stated intervals, with all the needful calculations and reductions, should be considered as an absolute requirement; and more particularly is it the case in the instance now contemplated, as it would form the least exceptionable, and perhaps the only possible substitute for "visitation" and supervision.

The numerical results might indeed be sent home from abroad, and be printed by deputy; but this cannot be done with the drawings, which will necessarily form a large part of the "results," and no one but the artist himself can efficiently revise an engraving after one of his own drawings.

Either therefore the astronomer must come to England occasionally, and see his works through the press, or he must have the engraver with him at the observatory, and the plate-printer too, for there is no good engraving to be performed without frequent proofs being pulled in the course of the work; and as sufficiently good engravers and printers do not exist at present in the Southern hemisphere, they would have to be sent out expressly for this purpose.

In place of engraving, we may, however, now take advantage of the improved photography of the present day on glass-plates, as a method of multiplying drawings with abundant accuracy and sufficient artistical perfection.

Photography may be dearer than mechanical printing; but considering that only a limited number of copies will be required, and that the whole expense of engraving will be saved, it may be found a saving on the whole.

I remain,
Your ever, &c.,
C. Piazzi Smyth.

Capt. W. H. Smyth, R.N., F.R.S.

Appendix.

Note on a Method of Constructing an Equatorial Mounting for a large Reflector. *By* C. Piazzi Smyth.

When the focal length of the telescope is short, the stand should be of cast metal, and ought to be placed under a revolving roof. But if a greater length be decided on, the dome may be dispensed with; for, according to the experience of Lord Rosse, the wind does not shake the larger of his several telescopes in a more rapid proportion than the surface, probably on account of the weight increasing with their size.

The observer himself, however, should be protected from the weather, as no accurate measurements can be taken by any one exposed immediately to the tempestuous gales which blow with clear skies at the Cape and other places in similar latitudes. The telescope tube moreover should be fastened at both ends, as in the altitude and azimuth stands at Birr Castle, so that the wind may have no leverage over the fixings.

Then by adopting a construction of wrought-iron plates, to be put together on the spot with red-hot rivets, as in the case of the Britannia tubular-bridge, a parallactic mounting for a telescope of any size, say 100 feet long, may be made in the following manner, and with the full preservation of all the above-mentioned conditions.

1st. Let the fundamental portion of the polar axis be a wheel

100 feet in diameter, with a very strong hollow rim made of plate-iron, 1 inch thick, with radial stops, and 7 feet broad by 15 deep (in the plane of the wheel). Four strong spokes, made also of wrought plates, to carry a cast and turned ring 10 feet in diameter, as the nave of this wheel, which will be also employed as the declination circle. The polar-axis pivots to be tubular, 3 feet in diameter, and to be fastened on the rim of the wheel by broad bases, the whole being bound together by tires of malleable iron put on red-hot.

2nd. To strengthen the above laterally, as well as to afford the means for clamping and giving slow motion in right ascension, let there be fixed to the back of it an "equatorial semicircle," of the same tubular construction and with the same radius, and let the driving clock act upon its periphery.

3rd. For further strength in inclined positions let there be two "six-hour quadrants," connecting the middle of the equatorial semicircle with the rim of the declination wheel at the polar-axis pivot points.

4th. The declination axis to be a conical tube 3 feet in diameter at one end, and 10 feet at the other, with a broad outward flange in addition, and long enough to reach from the outward face of the declination circle, through the whole structure, to the junction of the equatorial semicircle and the six-hour quadrants, there to be held in by a powerful screw.

5th. The telescope tube to be double, and of $\frac{1}{2}$ inch wrought iron plate, with longitudinal ribs riveted between.

6th. This cylindrical tube to be enclosed within a prismoidal one, by one whose flat surfaces may be attached to the flange of the declination-axis. The angular corners of the outer tube to be fitted at short intervals in its length with stops, so arranged with large central apertures as to form enclosed ladders, whereby the observer may climb up from the speculum end of the telescope to the eye-piece end. There the prismoidal tube may be expanded into a small room, which will screen the observer from the weather, and may be so ventilated up one side of the casing between the tubes, and down the other, that no heated air shall pass over the mouth of the telescope.

7th. The face of the declination circle will bear graduated arcs of turned metal for pointing the telescope by; and a rack edge, to be worked in by a pinion, which may be turned for the observing room, to give motion in declination.

8th. An arc of turned metal, with a toothed edge, is also to be fastened to the rim of the equatorial semicircle for the driving clock to act upon; and this being elevated on a pier to the level of the centre of the polar axis, and placed east or west so as to act at six hours' distance from the meridian, allows of an uninterrupted motion in every direction.

This new form of equatorial will thus be firm and stiff in every direction; its long polar axis and small terminal pivots at either end will ensure great truth of movement in right ascension. The driving clock will act with ease on so large an hour-circle, whose plane too,

passing through the bearing centre of the telescope, reduces the length of the polar axis, affected by torsion, to a zero. The telescope, moreover, is clamped at both ends, and to as firm a declination circle as could be desired; while the protection and security given to the observer, with the power of directing his instrument without descending from his post, the ease and certainty of setting to the faintest objects, and the command of the whole sky, uninterrupted by any mechanical difficulties of reversal on passing the meridian, or otherwise, render this parallactic mounting as convenient for use as it is safe and trustworthy, on account of its strength and its composition wholly of metal.

Mr. Airy to Mr. Bell.

Royal Observatory, Greenwich,
December 24th, 1852.

Dear Sir,

Since the meeting of the Southern Telescope Committee, I have repeatedly considered the subject of equatorial mounting of the telescope. And I think the probability of this being made practicable and efficient is so great, that I trust that the Committee may be induced to suspend any positive decision until they shall have discussed special plans.

I hope to be able to prepare a model in time for exhibition to the Committee; meantime I may explain that the principles which I propose to adopt are the following (of which the first and second have been long entertained by me as necessary for the safe and convenient use of a large reflector).

1. The mounting, as regards the support of weight of the telescope and the keeping of the same edge of the speculum always at the bottom, is to be strictly that of an altitude and azimuth instrument.

2. An equatorial construction is to be used, not for supporting any part of the weight, but for guiding the telescope.

3. This is to be so connected with clock-work that the telescope will move truly in hour-angle, without requiring any adjustment of the clock-work for different polar distances.

4. The eye-piece and micrometer-work are to receive a motion exactly corresponding to the rotation of the meridian as seen in an altitude and azimuth instrument; so that the micrometer-wire, once placed to make a certain angle to the meridian, will remain making the same angle with the meridian.

Believe me, dear Sir,
Faithfully yours,
G. B. Airy.

Thomas Bell, Esq.,
Secretary of the Southern Telescope Committee.

Dr. Robinson to the Earl of Rosse.

Observatory, December 28th, 1852.

My dear Lord,

Colonel Sabine has sent me Mr. Nasmyth's letter on the subject of the southern reflector, and your remarks on it.

If I comprehend Mr. N.'s drawing rightly, his polar axis consists of two parallel frames, united at top and bottom by two transverse pieces, between which the telescope is suspended. There seems no provision for seeing any part of the sky near the pole; and I think the axis would be peculiarly liable to torsion. The idea of turning the whole telescope round *its* axis, involves, I think, some difficulty. It must be provided with two systems of rollers, which under the open air may not always act well; and it may perhaps be easier to make the small speculum and eye-piece revolve.

I quite agree with Mr. N. in wishing not to shorten the focus beyond your proportion of 9 diameters. I have succeeded very well (15 inches) in 7·2, but with far greater difficulty than with 9. Indeed, even when the process failed so far that the whole aperture was not good, the proportional part of it was mostly excellent. I hope therefore that the 4-foot may not be attempted with less focus than 36 feet.

I enclose you a tracing (which return when you have looked at it) of two schemes which were discussed by Grubb and myself in reference to the southern reflector some years since. In that marked R, I proposed to make *the speculum box itself* the declination axis, providing it with journals and a circle, and to have a short and massive polar axis. The tube above the box was to be merely an open skeleton, framed so as to be stiff enough to support the ocular part and small mirror. This would bring the centre of gravity very near the speculum. Grubb did not like this, and suggested another marked G. The polar axis here has its upper bearing about 6 feet diameter, but is cut at top so as to let the tube pass in for reaching the pole. This would a little weaken the axis there, but from its great diameter the strength would be abundant. It would also interfere with the motion of the instrument at more than five hours from the meridian.

I think none can gainsay your remarks as to the advantage of an equatorial movement. Work such as we contemplate is facilitated almost beyond estimation, when the object to be drawn is kept immoveable in a given part of the field; and the fatigue of signalling or shouting to the assistants when the movement is by hand, absorbs no small portion of the observer's powers.

And I think your preference of the "German" form over the "English" admits of as little dispute; that one of the latter form *may* be steady if an enormous mass be given to its moving parts, is obvious, but it must be remembered that such mass increases most materially the difficulty of using it, and that the increased friction must cause great additional strain on the moving parts. It is probable that the metal of the Liverpool mounting, if applied in a way

similar to Mr. Cooper's instrument, would carry a three-feet reflector. I have been told it weighs six tons. Now the weight of Mr. Cooper's is—

	Cwt.
The equatorial part	25·95
Tube, object-glass, eye-tube ...	9·12
Counterpoise.....	12·42

Of the first the polar axis is about 18 cwt., or twice the weight of the telescope. Now I have been familiar with this instrument's working during many years; it is very easily moved, and perfectly steady. The shelter of the lower half of its tube by the surrounding wall, in my opinion, rather increases the power of the wind on the upper part to shake, as its pressure is unbalanced. In the case of a reflector, the influence of the wind would be much lessened by making its tube a skeleton, and I think that *instead of the counterpoise*, there might be put at the other end of the axis *an apparatus for supporting the observer*. *He* should be counterpoised so that the frame could be lowered to get him into his chair, then raised and clamped in his place. Mr. Cooper's equatorial cost, I think, about £400.

The Germans themselves are quite satisfied with Reichenbach's construction, and that of Munich is, I know, used in the open air. Their polar axes are solid and made of steel, but it is certainly better to throw the material into a tube. Short bearings seem to be the secret; it is scarcely possible to make a stronger axis than that of your equatorial, but the length makes it twist. Need a polar axis be longer than the mandrel of a lathe? I forgot to mention that Grubb is getting ready two equatorials for the Dublin Exhibition; one a model of that noticed above, with a 15-inch reflector, the other an improvement of Mr. Cooper's, with an 8-inch achromatic of his own grinding. I will report to you, or perhaps try them with you.

T. R. Robinson.

The Earl of Rosse.

P.S. Of course anything from Airy must be considered; but I don't expect much improvement on the equatorial. Before I had my equatorial mounting for the reflector, I found that with the old 9-inch Herschel which I found here, and its rickety stand, I could take no measures with the micrometer, and in 1830 I made an alteration, which may possibly be what Airy is devising. The telescope was set on a circle revolving on balls by a joint something like your 6-feet one, and was counterpoised. But the horizontal slide of Herschel's stand was connected with a circle, by means of which it could be inclined to the angle of position, and clamped there. This angle was got by a table of double-entry, of which the arguments were the azimuth and altitude, both given by the instrument. The slide-screw gave the movement; it acted very fairly as a substitute for the length of the rack, about 30 minutes at the equator; but, as you may suppose, I was not sorry to get a true equatorial. I really do

not see any insuperable difficulty about the mounting. A common turn-table with its edge on rollers would bear forty tons, and there would be no great trouble in setting strong standards on it to carry the tube. Remember, that one does not want the delicacy of a meridian circle here, only motion.

Mr. Nasmyth to the Earl of Rosse.

Bridgewater Foundry, Patricroft, near Manchester,
December 30th, 1852.

My Lord,

Having made out a more careful drawing* of what appears to me would be the most suitable construction for the proposed 4-foot reflector, I take the liberty to send it you, as it may so far tend to establish the best arrangement for such an instrument.

I am very fain to have it mounted equatorially, as there are such vast advantages in that construction for the steady observation of any selected object of which it may be desirable to make careful drawings, and as when once the instrument was set upon the object in question, the clock-work apparatus would do the rest. The clock would require to be of very considerable power, but I see no difficulty on that head. By having suitable means whereby the assistant could cause the tube to revolve in the square frame cage, the eye-piece can be kept horizontal at all times, which would also be equally advantageous for the easy set of the metal on its under edge-bed. The jointed cell for the metal is a very handy arrangement, whether for opening the clock for sliding in the disc of zinc as the protector, or for the easy removal of the cell and metal by truck and railway to the polisher when it is required. A truck with an elevating top would do this very handily. The three elevating screws would be found very handy in getting the cell into connection with the joint on the tube; the truck might be so arranged as to run direct into the polishing machine, and so avoid all the risk and trouble of transferring the cell from it to the machine. The tube would require a chain to retain its position when the counterpoise action of the cell and metal was removed. Pray excuse this very hasty letter, and believe me I am

Yours most respectfully,

James Nasmyth.

To the Right Honourable
The Earl of Rosse.

Mr. Lassell to Mr. Bell.

Valletta, December 30th, 1852.

Dear Sir,

I was preparing some reply to the circular of the Southern Telescope Committee, dated the 1st instant, which has been forwarded to me here, when I received your second circular of the 15th instant.

* May be seen at the Royal Society's Apartments.

It appears to me that the question now wears quite a new aspect, and it will be for the Committee to determine whether they will carry out their original purpose of erecting an eminently large telescope in the southern hemisphere with every appliance which their united wisdom can secure—or whether they will, as a preliminary step, elect an observer, and commit to him the conduct, arrangement and carrying out of the whole affair.

Concluding that they will adopt the former resolution rather than consign so great an undertaking to any single individual, however able and accomplished, I yet see all but insuperable difficulties in adequately realizing so grand a scheme.

If there were any manufactory in the world which could furnish completely mounted a 30-inch object-glass of similar quality to the 15-inch Munich glasses, I should say the best thing would be to give the order at once. But as that is not the case, the proposed reflector of 4-feet diameter is the best that can be chosen (in my judgement), though I would rather not shorten the focus beyond 35 feet; conceiving it to be difficult to get the figure, otherwise, good quite up to the edge; and it seems to me unwise to incur all the difficulties of a colossal telescope, and then be obliged to cut down its dimensions.

I know something of the efficiency of a 2-foot reflector, when brought up (I believe) extremely near to perfection in its figure, and furnished with a convenient and steady equatorial mounting driven by clock-work. It would not be worth while for the Committee to erect a telescope which should not be at least in comparative efficiency to this, *as two to one*; and in order to render the 4-foot equal to this it must have the same conveniences of mounting; for any deficiency in this respect will really have the effect of a reduction of the size and power of the telescope. I have seen a sketch of a plan of equatorial mounting by my friend Mr. Nasmyth, which is simple and well-calculated for such a latitude as the Cape; but I think it would never do to suspend the observer from the eye-piece; for the slightest motion of his body, such as the extension of his arms to adjust the focus, alter the polar distance, or turn the micrometer-head—even his very pulsation, would be so communicated to the telescope as to be visible in the image with the high powers proportioned to such a telescope. Besides, a very moderate breeze would equally prevent delicate observations, and therefore the telescope must have a revolving roof for protection from the wind, and affording a status for the observer. By placing the instrument a little out of the centre, 50 feet might do for the diameter of such a dome, and the few degrees of sky near the zenith thus sacrificed, might be recovered by a shutter, only to be used on requirement, which would extend the opening over the centre of the telescope when vertical. From the experience I have had in the construction of domes 15 and 30 feet in diameter, I am satisfied that the same principle of construction might be extended to 50 feet, and that no very formidable difficulty would present itself here.

The mounting and application of driving motion would be readily

executed by estimate according to plan, by any of our more intelligent engineers without any serious obstacle.

But the casting, grinding, polishing and supporting of the great speculum, constitute the grand problem the solution of which it is not easy to see. For the successful accomplishment of the most essential part of the work, there must be *perfect unity of design and execution*, and therefore it must, in my opinion, be executed entirely under the direction of a single individual, who will nevertheless be able to avail himself of any suggestions of others which he may deem judicious. I will mention some of the requirements which I think demand this condition. Two specula must be cast, 4 feet in diameter, and not much less than 6 inches thick, each weighing therefore about 3500 lbs.; and supposing the difficulties of casting and grinding got over without accident, there comes the greater difficulty of polishing and bringing up to the requisite accuracy of figure, and most especially, *of defining power*. For this I would submit no one is competent who has not had considerable experience in the art itself; the cleverest engineer would be here at fault; and indeed it is a process which has so rarely been attempted on a large scale, that I think it would not be easy for those best acquainted with the subject to say when the greatest practicable amount of perfection was obtained. It is obvious that some machine must be used, and that, in all probability, repeated polishings and trials must take place before the required accuracy of figure and defining power can be combined in their highest degree. Both specula therefore must be figured and polished in the first instance, examined, and the worst of them taken again to the polisher. For this examination with sufficient advantage, the entire mounting of the telescope must have been previously erected; in fact the whole telescope, dome and clock-work and all, must be erected completely on the place of operation, just as it would have to be in the place of its ultimate destiny, before a satisfactory final result can be secured.

Another formidable difficulty I have yet to mention—the sure support of the speculum to prevent its bending, and secure its uniform action in all positions. In addition to the system of levers on which the speculum is to be ground and polished, and which secures this condition when horizontal, another system, similar or equally efficient with that I have described in the British Association Memoirs for 1850, must be applied, to come into action as the telescope is depressed from the zenith, and prevent the speculum *cringing down* as it is turned on edge. Without this latter apparatus the image of a bright star gives indisputable evidence of the speculum taking an oval form, from the impossibility of bringing the rays from the ends of two diameters of the surface of the speculum at right angles to each other—to focus at once. Hence the intolerable annoyance which I am accustomed to call "crossing," visible enough (if the metal otherwise define well) on Jupiter or Saturn as well as on fixed stars. To apply this apparatus conveniently, the specula should have either a number of blocks, or indentures, cast on the

back; the latter would be best, in order not to interfere with the smooth grinding of the back, which would be desirable.

Supposing then the telescope at last erected complete, contiguous to the site of the workshop and pronounced perfect, would there not be some compunction at taking it down before some experiment of its magnificent powers on the heavenly bodies should be made? However, letting that pass, and looking to its ultimate object and destination, I don't see who is to manipulate with such a telescope but the maker. I believe that hitherto no very large reflector has done any good service but in the hands of the artist who constructed it. At least if there were an astronomer, *par excellence*, to conduct the observations, the maker must accompany him, and, I should say, take out the polishing machine and apparatus for renewing the surface in case of accident or deterioration of the lustre by exposure.

WILLIAM LASSELL.

To Thomas Bell, Esq.,
Secretary of the Royal Society, &c.

P.S.—The suggestion of a great altitude for the site is good. Perhaps *Quito* would do, but how is the telescope to be got there?

Sir J. W. Lubbock, Bart. to Mr. Bell.

March 12th, 1853.

MY DEAR SIR,

As I am requested so to do I offer the following remarks, with very great diffidence, not having had any experience either in making or using large instruments. Besides, almost every point is so ably handled in various letters from members of the Committee that the subject is pretty well exhausted. There are, however, one or two which have not been noticed.

1. Surely the Government will never authorize the establishment of a great national observatory at Quito, or any point not in our own territory. If, then, Australia is also out of the question by reason of the abnormal state of that colony, we have only the Cape to consider, and so that point would be disposed of.

2. Mr. Smyth attaches great importance to elevation; this I can quite appreciate; but the elevation which he contemplates will, I think, never be reached without disadvantages which will outweigh the benefit; and there are two other conditions I should consider as essential,—

1. The non-contiguity of the sea or of much standing water.
2. The non-contiguity of any manufactory or large town.

I confess the impression made upon me after attentively reading all the letters, and especially Mr. Lassell's, is that it will be impossible to carry out the plan with a reflecting telescope (I mean of 4-feet diameter); secondly, that it must be equatorially mounted and sheltered by a dome; thirdly, that *if* it can be done on the mag-

nificent scale apparently contemplated by the Committee, we had better begin by having it in the northern hemisphere first.

I apprehend (but perhaps I may here be in error) there is plenty of work at the Cape for an instrument of such comparatively inferior power as the Cambridge (America) telescope, and up to this point the erection of an observatory would present no insurmountable difficulties.

In Mr. Nasmyth's mounting, in addition to the other difficulties noticed in the letters, the chair of the observer would act at such a mechanical advantage, that I suppose it might affect the tube, but perhaps it is thought that a slight strain on the tube would not be injurious to the observation.

I am, dear Sir,

Yours faithfully,

Thomas Bell, Esq. J. W. LUBBOCK.

Sir John Herschel, Bart. to Mr. Bell.

MY DEAR SIR,

I am very sorry to have kept the papers relating to the telescope proposed to be established in the southern hemisphere for the observation of nebulæ much longer than I could have contemplated; but in truth I have been so much engaged of late that I have found all consecutive thought on the subject impossible, and can now only give what has occurred to me on their perusal in a very unconnected form.

I must, however, preface my observations with a distinct statement that I am by no means sanguine as to the success of the project—I mean as to the attainment of results at all commensurate in scientific importance with the imposing character of the undertaking as a national one, and the *very large* outlay of the public money which it will call for. It is by no means therefore as an advocate for the measure (on which, in an earlier stage of its progress, I have expressed my opinion more at large) that I have assented to my name being placed on this Committee, but that I may not be considered as disposed to withhold (should the project be persevered in) such slender information and practical suggestions as my experience in observations with large reflectors, directed to this object, may enable me to offer as to the kind of instrument and the mode of observation best suited to the purpose. It seems above all things necessary that, before proceeding a step in the affair, its movers should have before them a very clear perception of the sort of instrument required, and its adaptation to the work in hand.

There are only two constructions specially indicated in the papers which have come into my hands—those of Mr. Nasmyth and of Mr. Smyth. To the former of these it appears to me that there exist serious objections, as follows:—

1. The polar axis (the mounting being equatorial), as exhibited in the figures, however well trussed, cannot but have some degree of

spring perpendicular to its length, which will ensure tremor whenever there is any wind or any movement of the observer in his box. Although the telescope is supported on its centre of gravity, yet owing to the different lengths of its two portions on either side of the point of support, and the very different disposal of the weights, the propagation of such tremors through them cannot be synchronous, and must therefore result in momentary flexures to and fro of the tube, giving rise to oscillations in the line of collimation, and consequent tremulous apparent movements of the image.

2. Such a frame as that of the polar axis, divided down its whole length, and that length a considerable one, cannot but be liable to torsion, since, however well the sides may be trussed, they will ultimately act as two flat ribands. The effect of such torsion is conveyed *entire* upon the line of collimation; and as the upper and longer portion of the tube will be much more exposed to wind than the lower and shorter, I should expect the tremors so produced to be exceedingly obnoxious. The hold of the wind on such an apparatus, totally unsupported except on the centre of gravity, will be most formidable.

3. As the instrument deflects out of the meridian, the observer will have to look to the security of his position, and to work a mechanism to keep his box vertical. It does not appear by what mechanism this is to be performed. The mere weight of the observer and box will not do it, and if they could they must overdo it, and fall beneath the tube. I can imagine none free from objection, and the line of collimation will be kept in a constant state of unsteadiness and derangement from this cause, which may carry it out of the reach of small corrections.

4. The observer will have to balance himself in his box before elevation into the air by adding or subtracting counterpoises. He is insulated aloft, at an elevation from which he cannot descend without the aid of an assistant, at whose mercy he is, and who may be negligent or sleepy. His position would certainly not be an enviable one.

5. The speculum and its supporting frame hang on a chain by an oblique pull on the edge, being sustained also by a hinge below. I can imagine nothing more objectionable. An enormous power, many times the weight of the mirror, would be required to be applied to this chain to keep the mirror-frame tight home as screw-work would do.

I am hardly engineer enough to criticise Mr. Smyth's gigantesque construction of a 100-feet equatorially mounted reflector of riveted iron plates, which might perhaps have that sort of stability which any vast aggregation of materials may possess under moderate winds; though, from the extent of surface exposed to such gusts as prevail at the Cape, I should not be very confident even in this respect. But there are several points which may be mentioned as objectionable about it.

1st. There is no provision in it for keeping one side of the mirror always uppermost, and the observer in a vertical position. Such an

adjustment is essential, and I believe can only be adequately secured by the altitude and azimuth movement.

2nd. Mr. Smyth considers it sufficient to destroy the evils of heated air that the chamber at the mouth of the tube should be duly ventilated to keep the personal heat of the observer from passing across it; and, as regards the rest of the tube, he does his best, by a cellular structure of its *parietes*, and by enclosing it in an outer triangular case, to give a durability and uniformity to *its* temperature, which in my opinion must infallibly secure a continuance of that slow spiral movement of the internal air, by the interchange of air within and without, through the upper aperture which is the greatest enemy to distinct vision in reflectors.

3rd. The observer's access to his aërial chamber is by climbing up a sort of chimney, with ladder-steps in one angle of the triangular envelope. In his figure this chimney is $\frac{1}{11}$ of the internal diameter of the tube. For a 6-feet reflector this would afford a sufficiently roomy, though very inconvenient passage; but for a 4-feet size would allow barely room for a man to creep up, unincumbered with any books, drawing-apparatus, &c.

In the construction of a large reflector I would press the following points as essential to be attended to:—

1st. A very perfect internal collimating telescope, with means under the observer's hand of adjusting fiducially the line of collimation without the intervention of an assistant. In my "Cape Observations," the means of doing this most effectually, most simply, and to any required degree of precision, are pointed out,—subject of course to improvement in point of workmanship and mechanical arrangement.

2nd. A perfect support of *every point* of the mirror on an elastic bed. I am by no means entirely satisfied (speaking with all due respect) with Lord Rosse's ingenious contrivance of triplets. It is mechanically complex, weighty, and, after all, only supports a definite number of points. I should prefer a bed composed of a great many layers of horsehair-cloth, or frieze or felt, of such united thickness and softness that their total compression by the weight of the mirror should exceed by a great many times the minute amount of flexure undergone in the solid back-support on which they and the mirror both ultimately rest, *thereby annihilating the influence of such flexure on their supporting power.*

3rd. The mirror should be so sustained as to rest *against* and *upon* this bed, under any circumstances of inclination to the horizon, with perfect freedom to rise and fall, advance or retreat, without change of its parallelism, which may be accomplished by suspending it in the mode described in my "Cape Observations" (Introd. Art. xv.), in a jointed ring from a point above its vertex. In this, it must not rest on a single point at its lower edge, but should be supported over a large arc of from 60° to 90° of its inferior circumference on soft and compressible material, interposed between it and the ring. *Such a mode of support however necessitates the conservation of the horizontality of a fixed diameter of the mirror.*

4th. I should greatly prefer a skeleton tube, or one (if of iron plate) so pierced with large apertures as to allow of a completely free communication between the interior and exterior air throughout the whole length of the column, except perhaps in the very immediate neighbourhood of the mirror, for a foot or two from its surface, where it may not be needed. Long experience has satisfied me that the establishment of ascending and descending currents of hot and cold air in a long inclined tube, open at the top, chimney-fashion, is one principal cause of indistinctness of vision and deformity of image.

5th. The destruction of tremor is better secured by several cords of unequal tension, or by several steadying rods of unequal strength, weight and elasticity, than by one single one. It is to this principle that the exceeding absence of tremor in the 20-feet reflector used by me in my observations of nebulæ is attributable. The suspension of the upper end of the tube by a tackle of pullies, which, by their friction and the stiffness of the cord passing over them, never permits all its reduplications to have the same tension, causes them to vibrate out of unison with each other, and so to destroy each other's effect in communicating or perpetuating tremor.

A form of mounting has been indicated by the Astronomer Royal, in which an altitude and azimuth motion of the tube otherwise free, and permitting a range over the whole sphere, is limited to a given parallel of declination, by determining the distance of the lower edge of the orifice of the tube from a fixed point (which may be called the polar point) in a line parallel to the earth's axis, and passing through the common centre of the horizontal and vertical movements in the lower support of the lower edge, or in the centre of gravity of the instrument (if so sustained). If the difficulty of communicating a clock-movement to a tube so guided could be completely overcome, there would remain that of keeping the micrometer-wires in the field of view in a position invariable with respect to the parallel of declination. Mr. Airy speaks of a mechanism to this effect, but I have not had an opportunity of inspecting his model.

With an altitude and azimuth mounting, and with an object on the meridian, there could be no difficulty in communicating to the tube a clock-movement in the direction, and with the velocity corresponding to the parallel of declination, at least for a few minutes. Were this done, a photographic impression could be obtained, with this, as with an equatorial mounting.

I should rely on photographic processes to impress on paper a skeleton picture, or the images of *the stars only* which might accompany or be disseminated over a given nebula, or those of a cluster to be delineated, and trust to the eye to fill in this skeleton, for which purpose perfect fixity of the object in the field of view would be of no importance. I have no expectation that such processes could be applied to the actual representation of the nebulous parts of such objects, so as to give the true forms of nebulæ as they appear to the eye in the telescope. The visible contour of a nebula

varies from telescope to telescope with the illuminating power of the instrument, while that of a photographic impression (supposing one possible) would depend essentially also on the time of exposure, which acts in this case as increase of light would do, though, as it does not follow by any means (and the contrary is even true for bright lights) that (time of exposure) × (intensity of illumination) is proportional to (intensity of impression), there would be no security that the gradation of light in the several parts of the impressed image would be preserved as in nature.

On the other hand, I have no doubt that satisfactory photographic images of clusters of stars or the stellar assemblages in nebulæ may be obtained and multiplied, and become most valuable aids in the depiction of the latter class of objects. The telescope ought therefore to be provided with fit and convenient attachments for the necessary photographic apparatus, and the observer should be an expert photographer.

Whatever may be done in the way of delineating the forms of known nebulæ, the discovery of new ones by zone observations, whether conducted by meridional sweeps, or by the application of the equatorial motion, as well as the revision and perfecting of the places assigned to those already catalogued; in short, the formation of a complete catalogue of nebulæ, ought to form a leading feature of the observations to be made with such an instrument, and a large portion of the stock-work of the establishment.

As the construction and working of the telescope must be experimental in great measure, I should regard a rehearsal of the observations in England as a quite indispensable preliminary to their ultimate prosecution in the Southern hemisphere. It is quite of equal importance to place on official record exact delineations and catalogued places of northern as of southern nebulæ. The accuracy of such delineations could be tested on the spot, and officially reported on by a Committee appointed for the purpose, and confidence thus secured to the results subsequently obtained out of the reach of such a check. In a word, the northern hemisphere should be first dealt with, and that completely and effectually; and in the process, and within reach of amendment, the whole course of proceedings might be perfected, and the necessary experience acquired. Five years devoted to this (of which the two first would be chiefly occupied in *roughing out* the work, and getting the apparatus in trim, and the three last in a complete *resumé*) in the clear climate of the south-east of England would be time excellently well bestowed, and would shorten the period to be spent in the south.

I remain, my dear Sir,

Yours very truly,

Thomas Bell, Esq. J. F. W. Herschel.

Dr. Robinson to Mr. Bell.

Observatory, Armagh, April 10th, 1853.

SIR,

I have received from Mr. Weld a portfolio containing the Correspondence of the Telescope Committee, with a request that I would transmit to you any remarks which occur to me from its perusal. These relate to the construction of the telescope, its mounting, and its site. Before entering on them, however, I must express my belief that the opinion expressed by several members of the Committee as to the difficulty of making a 4-feet reflector is exaggerated. I have seen so much of Lord Rosse's operations, that I feel authorized to say this, *absolutely* for a 3-feet reflector; with high probability for one of four. And were the difficulty and the uncertainty even as great as those gentlemen suppose, *that is the strongest reason for pursuing this our purpose.* It is only by the reflecting telescope that we shall reach the remotest parts of the visible universe. There is no likelihood that an achromatic of 3 feet (the equivalent of a 4-feet reflector) will be made in the next century; if made, it must be of enormously greater cost, and will be embarrassed by the evils of flexure and the polarizing action caused by pressure. It is therefore specially desirable that the reflector shall be made as perfect as possible; and never was a better opportunity offered than now, when we shall be authorized to experiment under the guidance of Lord Rosse, Sir J. Herschel, Mr. Lassell and Mr. Nasmyth.

1. *As to the telescope*: I wish to suggest whether it may not be desirable to use Cassegrain's form instead of Newton's. No light is lost, for M'Cullagh and others have shown that the reflexion from metals is less intense at 45° than nearer the perpendicular. I have much experience of its power with 15 inches aperture; and can mention as an important practical fact, that slight errors in the large speculum can be corrected by the small one, which is ground and polished on a miniature of Lord Rosse's machine. This construction has the great advantage of not requiring any apparatus of a complicated character to support the observer, and of shortening the tube nearly one-fifth.

In reference to the mode of supporting the speculum, three methods are proposed. Sir J. Herschel's consists in letting it rest on an elastic bed, uniformly bearing it; Lord Rosse bears it by friction balls on a system of equilibrating levers behind, and a ring for the edge also connected with counterpoises; and Mr. Lassell applies a second system of levers behind the mirror, which sustain it at all inclinations. The first of these is liable to this objection, that the speculum cannot be polished on its bed, as it must be immersed in water; either of the others may be used; but I would direct attention to a remark of Lord Rosse's, that the speculum may perhaps be cast with ribs at the back so as greatly to diminish its weight. Were these arranged so as to intersect at the points of bearing, I am disposed to think half the metal might be saved.

Sir J. Herschel speaks strongly in favour of a skeleton or open-

work tube; which will both diminish its weight and remove the most injurious of the influences which interfere with the definition of reflectors, the circulation of irregularly heated air in the tube. In fact, owing to it they never act perfectly except when they are at the same temperature as the external air. If the tube be suspended at its centre of gravity, its upper portion has only to bear the small mirror, and in the Newtonian the eye-piece also, so that it needs no great strength; and its diminished surface will give but little hold to the wind.

I see no reason why any one diameter of the speculum should be kept always vertical, but rather the reverse. Whenever the telescope is vertical, all pressure is removed from the edge; now on depressing it, if the strain is restored to the same point of the edge, it is much more likely to produce a permanent change of figure than if its action is distributed over a wide range.

2. *As to the mounting*: I am decidedly for an equatorial, and see no insuperable difficulty in it, if, according to Lord Rosse's recommendation, we adopt the German construction, and possibly also that mixture of cast and malleable iron (a sort of steel) which is stated in the Report of the Commission on Railway Structures, to be $\frac{1}{4}$ stronger than cast iron. Sir J. Herschel's objections to the constructions proposed by Mr. Nasmyth and Mr. C. P. Smyth appear quite conclusive. That of Mr. Airy, of which I have recently seen a notice in the Astronomical Society's Notices, seems too complicated to be quite satisfactory; and I fear the bar apparatus at the mouth of the telescope would injure definition. I may mention in proof of this, that having supported the small mirror of the Armagh Cassegrain by three very thin radial bars at 120° asunder, I was obliged to remove them, as the stars were shown with three minute rays bisecting the angles.

Mr. Lassell's opinion is certainly of great weight, yet I continue to think that large telescopes should be worked in the open air; I have tried a 15-inch reflector and a 12-inch achromatic thus, and also under domes, and very much prefer their action in the first case. Nor need there be much apprehension from wind. Mr. Cooper's equatorial gives a fair test of this; the instrument has its tube half-screened by a wall, above which 13 feet rise, exposed to the unbalanced pressure of the blast; but it is very steady even in high wind. Whatever construction be adopted, should (if new) be tried first on a tolerable scale with a working telescope.

3. *Site.*—It must certainly be on our own territories, unless we reject all past experience. As to latitude, I should not like it near the equator: we have no information as to the practical working of equatorials there, and I think the circumstances which in Europe make that instrument more liable to unsteadiness than those of altitude and azimuth, would tell with more power when the polar axis is nearly horizontal. There also, the poles and polar regions of the sky can scarcely be observed. On this account Simla and even Ceylon are objectionable. The Cape is unexceptionable in this respect; but both from what is stated by Sir J. Herschel in the preface to his

observations, and from the letter of Mr. C. P. Smyth, I doubt its fitness as an observing station for a reflector. In particular the fine dust on which the latter lays so much stress (and which is so widely diffused in Africa that even at 1000 miles from its western shore it falls on ships) seems most inauspicious.

I wish we had some precise data to estimate the effect of height; the researches of Forbes, with which alone I am acquainted, refer to solar *heat* and not to *light*, and do not enable us to separate the absorbing action of vapour and air. I am inclined to believe that the first is much more opake than the other; and if so, elevations of five or six thousand feet are quite unnecessary, as the great mass of vapour lies below. From observations which I made at Munich (not 1800 feet above the sea), I think that 2000 will be fully sufficient; for I consider it absolutely necessary that an establishment such as we contemplate be easily accessible, and within reach of the appliances of civilized life; the very expense of its maintenance will be greatly increased if it is fixed on some remote table-land or insulated mountain summit; and the rapid communication with astronomers at home becomes scarcely possible. In respect to height, we must also keep in mind that the region of condensation is to be avoided. On the whole, I am compelled to think this element of very second-rate importance, or at least that it ought not to be predominant in deciding the question. If we wait till we know the ratio of a telescope's performances at the sea-side, and at 10,000 feet in the same climate, and then till we find a convenient mountain, we shall leave the performance of an important duty to another generation. If we can find a climate as good as that of Nice even at the sea-side, let us take it; and I am confident a 4-feet then will do more on nebulæ, than any existing achromatic on the top of the Himalaya (if an observer could live and work there). I would, however, suggest the Mauritius as possibly offering a favourable position. I believe the central plateau is about 2000 feet high; the climate is described as fine, and from its position to the *east* of Africa it will be free from dust.

Before concluding, I would direct the attention of the Committee to a remarkable passage in Sir David Brewster's letter, from which it appears that the application which, as President of the British Association in 1849, I made to Lord John Russell on this subject, never reached him, but was summarily disposed of by some irresponsible official. The lesson I hope will not be forgotten by any who, as the representatives of our great Scientific Societies, may have occasion to communicate with Government.

I have the honour to be,
Your obedient Servant,

T. Bell, Esq. T. R. Robinson.

Observatory, April 29, 1853.

P.S.—Regarding the size of the small mirror in the Cassegrain form :—the lowest power that will use the whole pencil of a 4-feet is 220; assuming this, and that the aperture of the small mirror and

the hole in the large, and the diameter of the field lens of the Huyghenian eye-piece are the same diameter, I find, assuming Lord Rosse's proportion of aperture to focal length $\frac{1}{9}$,

Field	15′ 38″
Distance of mirrors	29·14 feet.
Focal length of small mirror . . .	8·75 feet.
Aperture of it and diameter of hole .	0·76 feet=9·15 inches.

The diameter of the small mirror in the Newtonian form is only 4 inches; but the difference is insignificant in 4 feet; and besides the central part of a speculum is in practice not the best.

Grubb is grinding a 12-inch achromatic for the Dublin Exhibition; but I scarcely think he will be ready with it. The equatorial for it, however, is ready, and I think no doubt will exist as to its efficiency when it is seen. He grinds the lenses by a new and very simple machine, which acts without forming rings on the glass (Andrew Ross's great difficulty), and *if* it does as well for specula, will be an advance. I shall be curious to see how this object-glass acts; if it succeeds, there is some probability that he will try even larger sizes. But the *two glass discs* alone for a 24-inch would cost £1000.

E. J. Cooper, Esq. to Mr. Bell.

Markree Castle, April 29th, 1853.

Sir,

I beg to acknowledge the receipt from Mr. Weld of Dr. Robinson's remarks on the subject of the proposed Southern Telescope, written after his perusal of the correspondence contained in the portfolio forwarded to me, and returned by me to the Royal Society.

I have felt very great diffidence in offering any observations on this matter, particularly as my acquaintance with reflecting telescopes is limited; and I have hitherto confined the humble expression of my opinion to that branch of the inquiry relating to the site of the observatory.

However, as it appears to be the wish of the Committee that each member of it should be consulted, I do not think that it would be respectful to the Royal Society and British Association were I to withhold any remark that might occur to me. So far as I can understand the wishes of the Committee, they desire,—

(1.) A telescope not inferior to a 4-feet reflector, to be used principally for the examination and drawing of nebulæ.

(2.) The selection of a site on which such a telescope should be placed.

The first of these queries involves the considerations,—

a. Whether the telescope should be a reflector or a refractor.

b. The mode in which the telescope should be mounted.

c. The expense not only attending its first cost, but the permanency of its value, and also the maintenance of it in working order, together with the establishment connected with it.

(1.) As regards the selection of the telescope, it appears to me that the determination to have one of not less power than a 4-feet reflector evinced that the choice of the Committee had fallen upon a reflector in consequence of the difficulty, if not impossibility, of obtaining a refractor of equal light; and this apparent prejudgement caused my original hesitation as to taking any part in the deliberations of the Committee. I am free to confess that I see no prospect whatever of acquiring an object-glass affording light equal to a 4-feet reflector, even in its mean condition; but perhaps I may be excused for making some comparisons between the performances of the two instruments. Previously to Sir J. Herschel's voyage to the Cape, I sent him drawings of the great nebula in Orion, and of 51 Messier; and in his reply to my letter he stated his belief that his 18-inch reflector had more light than my 13·7-inch refractor; but that my refractor must be remarkable for its concentration of light. This opinion of Sir J. Herschel arose from my sketches of the nebulæ having been made under a high power, which, while bringing into view minute stars, invisible with a lower power, obliterated at the same time portions of the nebulæ which were seen with a lower power. Having compared my drawings under a low power with one he kindly sent me, I ascertained that with reference to light the instruments were nearly on a par. In his Cape observations Sir J. Herschel mentions his verification there of several minute stars seen by me, which his 18-inch reflector had not rendered visible to him in England. It would appear from these facts that a refractor giving equal light with a reflector, and in juxtaposition with it, would show stars better than the reflector. My belief has for a long time been that all nebulæ are clusters of stars, and in a great number of them stars are visible. It is of importance to note the position of these stars, for the purpose of detecting any dynamical change which may occur in the system of which they form a part. The exhibition of the spiral construction in some nebulæ presents a problem of intense interest to the physicist. It remains to be seen whether the practical astronomer will be able to furnish him with any materials towards its solution. The mere outline of a nebula will not be sufficient, as there might be an absolute permanency of form where the constituent star particles are in constant relative motion.

In point of definition I have always thought that reflectors had the advantage; but the drawings of the planet Saturn by Mr. Dawes and Mr. Lassell respectively (see *Ast. Nach.* No. 840),—the former by the aid of a 6½-inch aperture achromatic, the latter with a reflector of 2-feet aperture,—go far to fix an impression on the mind, that achromatics, at least of moderate size, can be procured to exceed reflectors of much higher relative power. It is also worthy of consideration that Mr. Lassell himself thinks, that if a 30-inch object-glass of similar quality to the 15-inch Munich glasses could be furnished, completely mounted, by any manufactory in the world, the best way would be to give the order at once. I cannot help agreeing with Mr. Lassell in this opinion, and am not prepared at

present to say that such an object-glass could not be produced at Munich. But I shall be met here by an objection on the score of the expense, even supposing that other objections should be withdrawn. Having heard that manufacturers estimate the probable cost of object-glasses as the cubes of their diameters, the expense of a 30-inch object-glass would no doubt be formidable; but we have already an estimate by Lord Rosse for a 4-feet reflector, amounting to no less than £6000; and it must be remembered that in addition to this sum for the telescope and its mounting, there must be an annual charge against the Treasury of no slight amount for the salary of the polisher. I would also deferentially submit that the permanency of the value of an achromatic is greatly superior to that of a reflector. I think that it will be admitted that the German mounting of an achromatic of the size proposed by Mr. Lassell, would be far less expensive than that of a 4-feet reflector.

Having said thus much on the subject of refractors as compared with reflectors, I turn to the remarks made by the several members of the Committee on the proposed 4-feet reflector. It does seem to me worth while to consider Dr. Robinson's suggestion to adopt the Cassegrainian instead of the Newtonian form, particularly if the latter should be held to involve the suspension of the observer at the mercy of his assistant. I also incline to the opinion of Sir John Herschel and Dr. Robinson in favour of a skeleton tube, although what the latter gentleman states of my equatorial is perfectly correct. I am also strongly in favour of the telescope being used in the open air, not conceiving that any astronomer would think of observing, even under a dome, during the prevalence of a hurricane. The equatorial seems to me to be the only form to adopt with a view to satisfactory observations. I cannot help considering photographic representations of the nebulæ as secondary desiderata. I fear that only a rough outline could be thus obtained, and I doubt much that very minute stellar points in the nebulæ would be distinguishable by this process. If my conjecture be correct, these pictures would only serve as rough outlines for future work.

(2). *Site of the Observatory.*—I have already stated in my former letter that I was disposed to recommend high table-land in a latitude as near 45° as possible. It is reasonable to demand what has led me to this choice. I have myself made observations both on the sea-coast and inland, in various places in Europe between 67° latitude and the southernmost part, and in Africa as far south as the second cataract of the Nile, viz. 21° 52′. The most favourable places for observation I found to be at Nice, on the coast of the Mediterranean, and at Munich. Throughout the valley of the Nile the weight of the dews after night-fall was so great that even an achromatic telescope could not be used at an early hour. In mentioning high table-land, I had in my mind the position of Munich, the observatory at which place is 1660 feet over the sea. The sky here struck me to be peculiarly dark, much more so than at Nice, and this position has what might be considered a disadvantage, ranges of mountains within a short distance. I therefore had no idea of ad-

vocating a position higher than, say 2000 feet, and I agree entirely with other members of the Committee in their different objections to a much higher situation. There is one circumstance with regard to a very high situation that does not seem to have been taken into account, except impliedly by Dr. Robinson, viz. the effect of the rarefied air upon the observer himself. I have my misgivings as to placing an individual in so different an atmosphere from that to which he has been accustomed. The preservation of his bodily health, and consequently his visual capacity, is surely of quite as much importance as the perfection of the instrument he has to use. So far as I am informed upon the climate and geography of Tasmania, I certainly think that it is probable that it would best fulfil the conditions that seem to me to be most desirable as to site.

I have the honour to be, Sir,
Your obedient humble Servant,
EDWARD J. COOPER.

Thomas Bell, Esq.
&c. &c.

Mr. Airy to Mr. Bell.

Royal Observatory, Greenwich,
April 30th, 1853.

MY DEAR SIR,

I have received, and now return to Mr. Weld, the correspondence of the Southern Telescope Committee up to the present time. In sending you some remarks in continuation of the same correspondence, I cannot avoid commenting on some of the proposals of my colleagues; but I trust they will understand that I have no wish to criticise any further than is necessary for conveying my own opinions.

I. ON THE GENERAL COURSE OF CONDUCT OF THE COMMITTEE.

1. I think that great attention is due to the letter of Mr. Lassell, and especially to his discussion of the question whether the Committee should decide every particular, or whether the Committee should give their confidence to an observer who should decide every particular. I certainly incline, as I understand Mr. Lassell does, to the latter course. And this was my feeling when, at the Ipswich Meeting of the British Association, I expressed my opinion that no public step ought to be taken till we had decided on the observer. I conceive that all that we can do at present is, to collect information to assist the observer; and all that we can do hereafter will be, to hear his plans and remark on them, leaving the adoption or non-adoption to him; and finally, when a scheme is fully arranged, to propose it to the Government.

2. I share in some degree with Sir J. Herschel the want of very sanguine hopes of success, at least for some time. And I think with

Sir J. Herschel that some time ought to be given to trials in England.

3. Lord John Russell's non-recollection of the former proposal, to which Sir D. Brewster and Dr. Robinson attach great importance, was, I suppose, simply the forgetfulness of one of the ten thousand proposals which come before a Prime Minister. I have not now the British Association papers; but I understood that the Memorial was addressed to Lord John Russell, and in that case (as it was not lost) it certainly went to him; and I believe that a reply came from the Secretary of the Treasury, and in that case it was undoubtedly discussed at the Treasury Board.

II. On the Site.

4. The station which appears to satisfy best the desires of some Members of the Committee for table-land elevation with south latitude is the Mauritius. But its latitude is low (22°), and this, as I understood verbally from Lord Rosse, would be very injurious to the observation of South Polar Nebulæ.

5. As very great importance is attached by some Members to the comparatively untried circumstance of elevation, I would submit whether it would not be desirable that experiments should be made with a pretty large telescope specially for this purpose. If such a station as is afforded by a Scotch mountain would suffice, I would propose the summit of Ben Lawers as preferable to that of any other mountain that I know. The ascent to within 100 or 200 feet of the top is very easy, up a grassy slope without bog or crag, where a horse and cart could go all the way; and there is a comfortable sheltered hollow close to the top, in which I found the Ordnance party encamped the last time that I was in Scotland.

III. On the Telescope and its Mounting.

6. In the only detailed plans which have been prepared for the Committee (Mr. Nasmyth's and my own), it is proposed that the observer should be suspended to the end of the telescope, if Newtonian. Some Members consider this likely to cause unsteadiness. It is much to be wished that Lord Rosse would institute decisive experiments on this point.

7. It appears to me very desirable that, before we decide on any plan for operations out of England, we should determine by experiment in England whether photography can be applied, either to nebulæ or to stars. For the special purposes of this Committee, as well as for astronomy in general, I do hope that Lord Rosse may be induced to experiment on this.

8. I think that Dr. Robinson's proposal of the Cassegrain construction is well-worthy of consideration. In reference however to the possibility of photography, I make the following remark (possibly unimportant). Supposing it likely that many photogenic rays might be lost at a second reflexion, I had arranged means in my model for receiving on the photogenic plate the image formed *immediately* by

the great mirror. If this be done with the Cassegrainian, the tube cannot be shortened as Dr. Robinson proposes. Moreover there will be loss of light, for the Cassegrain small mirror will probably be larger than the Newtonian small mirror, and cannot conveniently be made of silver as the Newtonian can. On the other hand, the Cassegrain gives great facility for the location of the observer, especially in my plan.

9. In my model, I exhibited a gallery or ladder for access to the observer's place, if suspended at the mouth of the tube; so that there is not really personal danger, or risk of being left to perish by a negligent assistant.

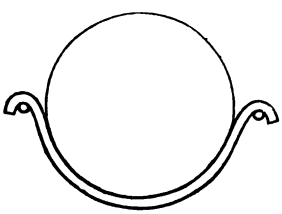
10. In deciding between different plans of mounting, I attach great importance to the holding the telescope by its *end*.

11. I should think a skeleton tube, with spiral braces, very desirable.

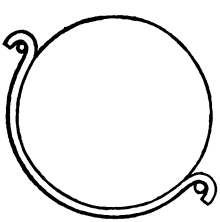
12. The evidence of Dr. Robinson and Mr. Lassell seems to show that a 4-feet mirror cannot safely be made with focal length less than 35 feet. This will require serious consideration.

13. I think Dr. Robinson's remarks are conclusive on the advantage of Lord Rosse's mode of supporting the mirror; yet I am sorry not to take Sir J. Herschel's.

14. Dr. Robinson remarks that there is no need to keep the same edge of the mirror downwards, and that it may be even injurious. As regards the abstract condition of a mirror, I have no desire to question this; but as regards the inquiry before us, I think it is founded on a misconception. It is not abstractedly the *mirror* which Sir J. Herschel and myself wish to preserve with the same line always horizontal, but the *apparatus that supports it edgewise*. Suppose (as in Lord Rosse's and Mr. Lassell's telescopes) the edge of the mirror is carried by a hoop or chain, thus,



then if the telescope were twisted so that the mirror and its hoop-support should be in this state,



the definition of objects would be absolutely ruined.

15. Dr. Robinson has remarked on the injurious effects produced by bars in the mouth of a small reflecting telescope, apparently 9 inches aperture. Supposing that in a large telescope the arrangement of the bars is *similar* and their thickness *proportional*, then the measure of the diffraction phenomena *in angle* will be inversely proportional to the diameter of the mirror *in linear measure*; and thus a disturbance of image, which would be extremely injurious with 9 inches aperture, will be absolutely insensible with 48.

16. Professor C. P. Smyth's proposed mounting appears too large for us to entertain at present.

17. I contemplate a telescope in the open air as best.

IV. On the Exhibition of Results.

18. I do not deny the importance of Professor Smyth's proposal for engraving on the spot; but I am far more disposed to encourage the preparation of very careful drawings, and trust to fortune for engraving them, as far as might be necessary, in England.

I fear that I have not preserved very strict order in the arrangement of my remarks, but I hope that they may be intelligible.

I am, my dear Sir,
Faithfully yours,
G. B. Airy.

Thomas Bell, Esq.,
Secretary of the Royal Society.

Dr. Robinson to the Earl of Rosse.

Observatory, Armagh, May 6, 1853.

My dear Lord,

I have read the remarks of Mr. Airy on the Telescope correspondence, and concur entirely with many of them. Others however appear to me less convincing; and though I feel strongly how weighty his authority is, yet as my acquaintance with you has made me familiar with large reflectors, and a tolerably long life given me some experience of the ways of men, I submit to you my reasons for questioning in the cases where we differ.

1. Mr. Airy would appoint an observer, and let him decide every particular. Nothing is gained by this but delay. The Committee *must* fix the nature of the work to be performed; they are the proper persons to fix also the means of doing it. Whether achromatic or reflector be used, the present Committee possess more knowledge of the subject than can be found in *any one person*. Let us provide the telescope, choose the site, and an observer will not be wanting. *If Britain cannot furnish a qualified person*, let us carry out free trade and seek him at Berlin or Poulkova.

2. Mr. A. has not very sanguine hopes of success. I have, and for this reason:—your papers show how much a 3-feet adds to the knowledge of the Northern nebulæ, which had been obtained by an 18 in., even in (*pace dixerim* !) a bad-observing climate. Assuredly

a 4-feet must do more for the Southern in the worst station than has been suggested.

3. My reference to Lord John Russell was meant merely as a caution to guard in any future application against irresponsible advisers of the Minister. The Royal Society's and British Association's recommendations should not be disposed of as they were in that instance.

4. Mr. A. thinks the Mauritius's latitude too low for South Polar Nebulæ. Our primary object is, I believe, the re-examination of Sir J. Herschel's Southern Nebulæ. Now in his catalogue there are but two whose altitude is under 30° at Mauritius, and one at that limit; and since your 3-feet is able to resolve part of the Orion nebula (at nearly the same altitude), *à fortiori* in the finer climate and at the greater elevation of the Mauritius, even it would be far more effective.

5. I like Mr. A.'s notion of Ben Lawers, and think it would be a proper object for the Royal Society to examine. The observations should be made at the base as well as at the summit, and the test should be the resolution of nebulæ. That of double stars is a different matter, being sometimes aided even by fog. But this and similar matters *need not delay the construction of the telescope.*

6. Mr. A. speaks of only two plans for mounting, very properly rejecting Mr. P. Smyth's as too vast for us. But I think you proposed the German form, and I suggested Mr. Cooper's instrument as the model. At all events I do so now, and think that with the use of "Stirling's toughened iron" and short bearings, it will fulfil all necessary conditions. I may mention that Mr. Grubb has one with some improvements in the Dublin Exhibition, intended for a 12-inch achromatic of 20-feet focus. It seems very strong, and when you (and I hope others of the Committee) see it, you will be able to judge whether its type might not be preferable. Experiments on suspending the observer need not delay the preparation of the speculum.

7. The same may be said of photography; *it is not an essential part of the plan,* and experiments respecting it need not delay the progress of other matters; the effect of metallic reflexion in absorbing the chemical rays may be determined in a day But I must add that I expect nothing from photography for nebulæ; it may give the places of small stars, but this object can be as well attained by attaching a camera lucida to the eye-piece.

8. In a 4-feet Cassegrain the small mirror should be 9 inches diameter; you I think would make it 4 in a Newtonian. The difference of light is unimportant. I do not know why Mr. A. thinks the small mirror of the Cassegrain cannot be made of silver. A mere plating is alone required, and the curved figure is more easily obtained than the plane.

10. The end support is good and steady; but it remains to be proved that sufficient steadiness cannot be obtained by supporting the telescope at its centre of gravity, especially if that centre be brought (as it may by using a skeleton tube) very near the speculum.

As far as I can judge from description, in Mr. Lassell's telescope the centre of gravity is about 6 feet from the end of the box; and I have not heard that he complains of tremors. It is, however, under cover; but, on the other hand, I should expect the mounting to be as firm as Mr. Cooper's.

14. The edge-support of the speculum which I use, and in reference to which my remarks were made, is not a semicircle, but an entire flexible ring attached to the box by three screws, two of which are acting in any position. The action is very satisfactory with 15-inch mirrors. But if Mr. Lassell's second set of levers be adopted, the edge support presents no difficulty.

15. I have great hesitation in questioning any statement of Mr. A.'s about diffraction. Still I would submit to him whether the reasoning in his investigation on the rings (the only one on this subject which I have seen) fully bears out his conclusion, that the diffraction *of a system of radial bars* is inversely as the aperture. The cases are not quite analogous; but in your *divided* 3-feet speculum, the lines of separation provided every star above the 6th magnitude with a cross, and a single packthread across the box of the solid one gave Rigel two tails. You can, however, easily fix on the 3-feet such a system of bars and try the effect. But even were this objection shown to be invalid, I still prefer the German mounting to that proposed by Mr. A. as far simpler.

On the whole, then, I am more hopeful than Mr. A. The most important of the difficulties which he suggests can only be verified by trial; and, if they be examined fairly, will be found not insuperable if we once set to work. What are these difficulties in comparison of those which *you* had to encounter? Have we not the benefit of your experience, and may I hope also of your assistance? For I will not disguise from you that I think our best chance of success depends on your permitting us to cast and polish the speculum at Parsonstown under your eye; and to mount and try it there (a part of the scheme which all seem to think desirable) before sending it to its destination. The mounting can be constructed by any great English machinist, and easily forwarded thither; and if the Committee share this hope and you will consent, I am certain that we shall have complete success. Of course we should reckon on some preliminary trials as to the form of the telescope and the mounting of the speculum.

Believe me, yours ever,

T. R. Robinson.

The Right Honourable
The Earl of Rosse.

May 11, 1853.

Memorandum.—With respect to the concluding remarks of Dr. Robinson, I beg to observe that I should be most happy to afford the gentleman engaged in constructing the instrument all the assistance in my power in the *shape of information*, and also by lending him

any apparatus I might have suitable for his purpose, should he think it worth while to avail himself of it. I should not *on any account* wish to interfere in any other way in the construction of an instrument undertaken by the Government. Besides, I think it would be but fair that the gentleman who had undertaken the task should have the whole credit of it.

ROSSE.

Extract of a Letter from Sir William Denison, Lieut.-Governor of Van Diemen's Land, to Colonel Sabine, dated Jan. 21, 1853.

"Capt. Kay has just sent me a copy of his orders from the Admiralty to break up the magnetic observatory at this place and return to England, unless indeed I would take the instruments and appoint an observer to carry on such observations as might be desirable:—now I am most anxious to maintain an observatory here, even for the moral effect it would produce, were there no special material benefits to be derived from it; but when I see that, in your address to the British Association, you laid great stress on the establishment of an observatory in the Southern Hemisphere, I have great hopes that I may reckon upon your support to a proposition which I have made to the Secretary of State to keep up and extend the present observatory. We want a fixed point with which to connect our triangulation, which may be carried across Bass's Strait and along the coast of New Holland so as to determine hereafter the length of an Arc of 20° of latitude and upwards: we want means of determining our time, and of rating chronometers for our shipping, in which the Colony has a great interest. I have the means of erecting cheaply all the buildings which might be required, and the cost of establishing an observatory would not be much. I have therefore accepted the offer of the Admiralty, and have appointed an assistant to take such magnetical observations as Capt. Kay may think desirable, and I trust that you will exert your influence to make the establishment as perfect as possible.

(Signed) "WILLIAM DENISON."

Dr. Robinson to Colonel Sabine.

Dublin, June 30, 1853.

I have communicated with Mr. Grubb as to his undertaking the construction of a 4-feet Reflector, and after discussing with him the whole matter, I think that he will be quite able to do it. With the exception of Mr. Nasmyth, who at present is too deeply engaged to wish to attend to such a task, I know of none at all so likely to succeed in this work as Mr. Grubb, especially if assisted by Lord Rosse's counsel and presence at the casting of the speculum. His premises are large enough to permit the instrument to be mounted on them, and he has a lathe of sufficient power to form the 4-feet

tools and the axis of the equatorial. He would make this after that modification of the German plan which has been tried by Mr. Cooper on one of his own, of which he will have a model ready when Lord Rosse comes to Dublin. In casting the specula he would leave a small aperture in the centre to try them as Cassegrains, which will not interfere with their employment as Newtonians, but which can be enlarged to give the full field if the former construction be preferred. In that case he would make the small mirror spherical, as more easily repolished if made of silver, and correct the larger one to it. As to costs, he *thinks* the instrument, two specula, a polishing machine, and one horse-power steam-engine, could be furnished for £5000. As however several things must be experimental, I think it would be safer to ask for Lord Rosse's original sum of £6000, with the understanding that the expenditure should be kept as low as possible. He thinks also, that with Lord Rosse to refer to in case of difficulties, the work ought to be completed in a twelvemonth from its commencement; the chief doubt being the loss of a month's annealing in case the first casting fails. Of this, from my acquaintance with Lord Rosse's process, I have little fear.

I would urge strongly on the Committee the propriety of making this offer of Mr. Grubb the base of their application to Government: if he be placed under the supervision of a small committee of practical men, with Lord Rosse as Chairman and Treasurer, I am confident he will do the work in a way that will prove satisfactory. If Mr. De la Rue would consent to be joined in this Committee, he could also give valuable aid; and if the Committee think that I can be useful, I shall be quite ready to assist.

While the telescope is in progress, the inquiries suggested by Mr. Airy and others as to Photography and elevation of site may be instituted, and when it is completed it can be tried by the Members of the Committee so fully, that we may be sure of its being a perfect working tool before it leaves the kingdom. All this presents no real difficulty, and the subsequent arrangements can be easily settled.

Yours ever,

T. R. Robinson.

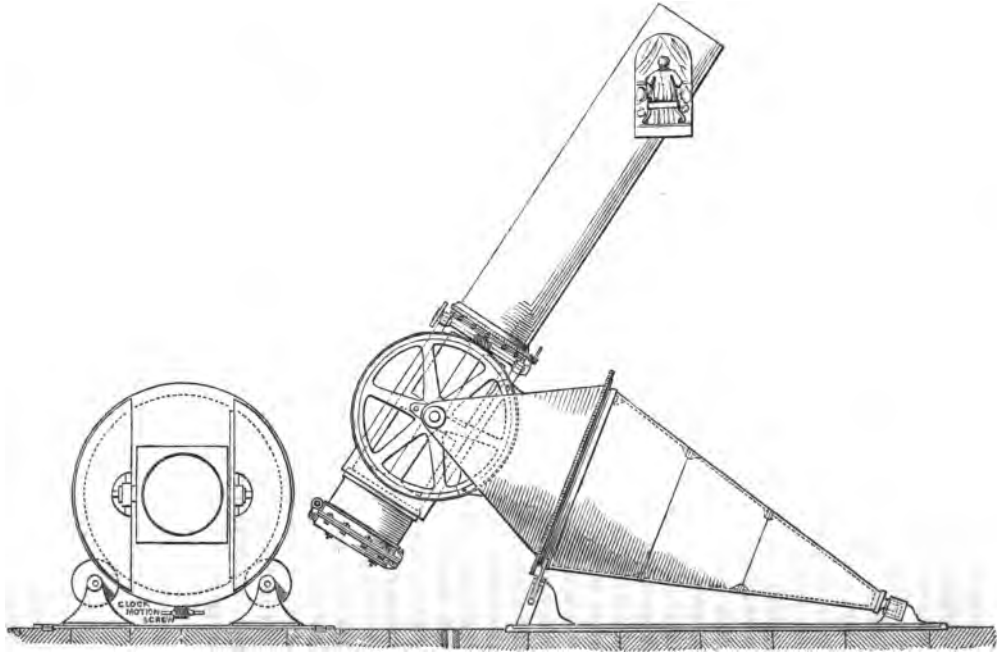
Mr. Nasmyth to Mr. Bell.

Fireside, Patricroft, July 2, 1853.

Sir,

I much regret that it will not be in my power to be present at the meeting of the Telescope Committee on the 5th July.

Since I addressed the Earl of Rosse on the 15th and 30th December 1852, I have had the subject of "the most suitable mounting" for this great instrument often before me, and have endeavoured to give it my most careful consideration. I now send you, for the consideration of the Committee, a rough sketch design of a system


CLOCK
MOTION
SCREW

of mounting (*genuine equatorial*) which I feel somewhat confident only requires to be substantially carried out to realize all the objects in view, as, while possessing *perfect equatorial motion*, it would be found to be quite steady and manageable, and by reason of the great simplicity of the entire arrangement, it would not in any respect prove difficult of construction or unduly costly.

It will be seen that I have based the entire arrangement on that which Mr. Lassell has carried out in his 24-inch Newtonian equatorial reflector, the general convenience and excellent performance of which leaves little or nothing to be desired, except it be in the increased rigidity of the trunnion standards, which in the case of the design now submitted by me to the Committee, I propose to form of two *half* cones of cast iron, cast of suitable thickness, so as that each be of adequate rigidity; a point which, by the form I propose to give them, will be attained in a high degree of practical perfection. These two half cones being most accurately fitted to the surface of the great hour-circle plate, and secured to it with suitable bolts, while this same hour-circle plate is in like manner fitted and bolted to the end of the great polar axis cone (which can be made of *any* required degree of solidity and stiffness), would yield a support to the telescope such as I incline strongly to think could not be surpassed in rigidity by any other arrangement.

Adopting in like manner the admirable system which Mr. Lassell has carried out of enabling the tube of the telescope to revolve within the trunnion cage so as to enable the eye-piece to be kept horizontal or in one constant parallel with the horizon, my favourite plan of suspending the observer's seat to a swivel-joint, whose centre of motion agrees with that of the eye-piece, becomes a matter perfectly easy to be practically carried out; and on this part of the arrangement I feel no small degree of confidence, that, had it only a fair trial, the result would *in every way* confirm the expectations I have formed of it, and prove to be in practice the most comfortable and appropriate, and in all respects the most convenient system of observing with the proposed great instrument.

With respect to the construction of the tube, the more nearly we can approach to the skeleton tube system the better, so as to get quit of the serious evils which result from currents of air in a close-sided tube. A system of parallel bars united together by cross and cross on right and left hand spiral bars, would, I doubt not, yield a sort of lattice work tube of great rigidity, and by permitting a free passage of the external air, in a very great measure remove the evils which more or less attend the use of an entire tube. *The only difficulty* I foresee in the employment of such open work tube is the letting drops of dew fall down upon the face of the speculum; and I must confess I do not very clearly see my way out of this difficulty, as the quantity of such water resulting from the dew will be found greater than most persons who have not had experience in open air observation are aware of. Were the lattice bars of such a skeleton tube wrapped round with some very bibulous cloth so that the dew might *cling* to them and be conducted down and out at the bottom

end of the tube, the risk of dew drops falling on the face of the speculum would be very greatly reduced.

To return to the question of the rigidity of the proposed system of mounting. As *any* amount of rigidity and solidity *can* be given to a polar axis formed as represented in the drawing, and the same unlimited rigidity and solidity can be given to the great hour-circle plate, and in like manner the same with respect to the two half or segment cone trunnion standards, and as this rigidity will be maintained *in every* position of the telescope, I feel confident that in that department of the affair there is no need for doubt or hesitation; and as the forms of these parts are most easy of production, and can be brought to the highest degree of accuracy of fitting by the most easy and simple class of workmanship, I trust they will recommend themselves to the Committee.

The mode which I have indicated of supporting the large end of the polar axis, namely, by causing its accurately-turned bearing (seen at the back of the hour-circle plate) to roll and rest on the edges of two anti-friction wheels, would be as perfect in action as it is simple and easy of execution. This again is one of the arrangements which I have borrowed from Lassell. There are very many minor details which I need not fatigue the attention of the Committee with at present, such as the means of giving rapid movement to the telescope when passing from object to object, or when the observer is taking up his position to work on any selected object, or on returning to *terra firma* after a four or five hours' spell; for if *real work* is intended to be done with this instrument, the observer would not need to move from his object for the above-named time *at least*; but I trust the state of mechanical knowledge of 1853 would supply him with the means of leaving work whenever he desired to do so, assistant or no assistant present notwithstanding. As I consider the plan of suspending the observer's seat to the eyepiece part of the tube is one of the grand features of my proposed arrangement, I hope the Committee will not cast it aside upon any grounds short of *actual experiment*.

I am, Sir, yours very respectfully,

Thomas Bell, Esq. JAMES NASMYTH.

Note.—In page 20, paragraph 5, of Printed Correspondence, Sir J. Herschel appears to have misunderstood the use and intention of the chain indicated in my first design. The object of the chain and small windlass is simply to bring the edge of the hinged cell up to the face of the end of the tube, and I know from practical experience that this is a most handy arrangement. The cell is then made secure to the end of the tube by three or six bolts, which maintain its position with the utmost security and correctness. The edge surface of the cell and that of the end of the tube being turned perfectly true, all that is requisite to bring the metal into the most perfect adjustment is to tighten up the holding screws which secure the contact of the edge of the cell with that of the end of the tube.

William Lassell, Esq. to Mr. Bell.

Starfield, 2nd July, 1853.

Dear Sir,

I regret that having only just returned home after my long absence, I cannot with any measure of convenience attend the meeting of the Telescope Committee on Tuesday next. A perusal, however, of the correspondence upon the subject has induced me to offer you a few comments.

1. With respect to site. Is Malta altogether out of the question? Down to 30° south declination, all objects can be advantageously observed there in respect of *position*; and, as regards climate, I fear a statement of the bare truth might be thought extravagant. My experience of the Maltese sky compared with the published reports of that of the Cape, lead me to conclude that the quantity of clear atmosphere in Malta is much greater, and that the quality of it generally is at least equal to the best at the Cape. As a lengthened trial of the telescope in England has been suggested, might it not be removed to Malta? and, if desirable afterwards to observe the stars down to the South Pole, it might be removed to the Cape. Supposing the instrument once perfect and satisfactorily mounted, its transit would present no very formidable obstacle.

2. The comparative merits of glass and metal, especially regarding *permanence*. I have no doubt the latter can now be made as durable as the former; in support of which I mention the following facts. My two-foot speculum I have within the last day or two replaced in the tube, in apparently as perfect a condition of lustre as well as figure, as when taken out for its journey to Malta. It has of course been packed and repacked, and has undergone two voyages and various cartages and transhipments; beside several months' exposure in active service under a roof not perfectly tight, and which was not able to defend it absolutely from spray during heavy storms of wind—nor from the almost incredible deposition of moisture during the occasional visits of the Sirocco. Whereas, on Merz's prism of heavy flint-glass, though never exposed except during observation, an acute eye may detect some *slight* deterioration of the surface; so that, in fact, the metal seems at least as invulnerable as the glass. To ensure this property, however, of the metal, the alloy must be carefully compounded by an assaying process before the casting is made.

3. It would certainly be well to have a skeleton or open work tube, but I do not think that the centre of gravity can therefore be got any nearer the speculum end of the tube. The skeleton tube, to be stiff enough, will probably weigh as much as a continuous one of sheet iron.

4. In common with others, I have frequently observed that the sky is not unfrequently in a very fine state when the wind is so high that observation without a roof is impossible, and therefore I think a dome should be contemplated. Indeed, the action *upon the observer*, of even a moderate breeze, especially if at all fitful, is too disturbing for delicate work. By leaving an opening between the top of the

circular wall and the base of the dome for the air, but not for the weather, and by making the quadrantal opening sufficiently wide, the atmosphere *within* will be, I believe, sensibly free from current, and as uniform in temperature, as *without*.

5. Mr. Cooper's conclusion, from the very similar pictures of Saturn made by Mr. Dawes and myself, that the telescopes must be nearly equal in efficiency, is a very natural one if drawn solely from the exhibition of this object. I would, however, remark that the planet Saturn is perhaps of all objects in the heavens that which is most favourable for the achromatic, and least so for the reflector. In Mr. Dawes's telescope I believe every perfection exists which can be included within its dimensions, and the observer, the whole world knows, cannot be exceeded. Saturn is so bright an object, that, where definition is perfect, the magnifying power may be carried on very disproportionately, especially if some care be taken to preserve the sensibility of the eye, even until there is only just light enough left to discern the phenomena; and in this way the disturbance of the air is reduced to a minimum. Mr. Dawes has occasionally used with advantage a power of 585, my common observing power being 650—not proportionate to the illumination. The atmospherical disturbances, if according to the areas, would be as 57 to 4 against the reflector, and on such an object would so increase by further magnifying power, that no advantage would be gained by applying it. Refractors, supposed equally perfect, will always have the advantage over reflectors of equivalent light, because the area exposed to atmospheric disturbance is less. The limited size of the former, however, soon places them *hors de combat*. If the comparison of the two telescopes in question be made by directing them to other objects than Saturn, such as Uranus and Neptune with their satellites, or the most difficult double stars, e. g. γ and η Coronæ, δ Cygni, ω Leonis, &c., their performances will be much more nearly in the ratio of their dimensions.

6. Provided the site for the proposed telescope has a latitude not lower than 35°S., I have no doubt that it might be well and efficiently mounted on the plan of my large equatoreal. It is almost exactly a copy of the mounting of the nine-foot equatoreal, described in the twelfth volume of the Memoirs of the Royal Astronomical Society. As it was constructed for the latitude of $53\frac{1}{2}$, I was a little apprehensive on taking it to Malta; and I at first used it there with great caution, and had wrought iron braces prepared before I ventured to turn the telescope six hours from the meridian. On careful examination, however, I found these quite unnecessary and very shortly removed them, as they interfered with the convenient working of the telescope. Curiosity, as well as a desire to feel secure, led me to measure the deflection of the "uprights" when in this position, and I found it barely sensible, certainly not a tenth of an inch. The distance of the centre of gravity from the end of the telescope, or the distance of the journals in the upper ends of the uprights from the box or frame in which it turns, in other words, the weight on the flange of the cone, is seven feet. The weight of the telescope-tube with its contents as ready for observation, including the square

journals, is very nearly a ton. In the use of the telescope in this trying position there was no sensible tremor or instability. From the small amount of flexure which took place, the load was evidently *far* within the breaking weight, and therefore perfectly safe. The axis *might* very well be only six feet from the end, as counterpoises are necessary at the object-end to remove the centre of gravity so far as seven feet from the speculum. The reduction of one-seventh of the length of the uprights with the same base, would of course add materially to their strength. With this alteration I am of opinion that it would only be necessary to double (generally speaking) the linear dimensions of the parts, to produce a mounting quite capable of carrying efficiently a four-foot speculum. Adopting the cubical ratio, and taking the weights of the several parts of my mounting as a standard, the weight of the hollow cone or base-piece (cast in two parts if needful) would be seven tons, and each of the uprights one ton and a half. The construction of the uprights, as well as the upper part of the cone, being on the edge-bar principle, it is scarcely possible to say what strain they will not bear, if the iron be good and care be taken in the castings that the various parts cool equably. To secure this was an important point in the construction of the patterns of the castings of my mounting. If it should be thought desirable to consider this plan at all for the great telescope, I would suggest some slight variations of form to add to its security; but I fear I am prematurely entering too much into detail.

7. One word about Cassegrainian. My experience is limited to a number of trials of a telescope of this form of 7¼ inches diameter, and a Newtonian of 7¼ inches, both my own workmanship. The result was that the Cassegrainian was slightly inferior in light to the Newtonian; but was in perhaps the same degree better than when, by the substitution of a concave small mirror, it was changed to the Gregorian form. On test objects, such as engravings and watch-dials, and sometimes on planets, the Cassegrainian especially, and sometimes the Gregorian, seemed to beat the Newtonian; but the Newtonian was always superior with the higher powers and on double stars. As various figures were given to the respective metals (or at least their surfaces were repeatedly changed) with generally the same mean result, I attributed the observed difference solely to the construction and not to the workmanship.

The application of the system of levers for preventing flexure on edge, requires certainly that the same diameter of the mirror be always vertical, or nearly so. This is accomplished with perfect ease in my telescope by rotation of the tube on friction wheels in the square frame carrying the axis of polar distance.

With serious fears of having trespassed much too long upon your time,

I remain, dear Sir,

Very sincerely yours,

WM. LASSELL.

To Thomas Bell, Esq., Secretary of the Southern Telescope Committee, &c. &c., London.

Lord Wrottesley to T. Bell, Esq.

20 Chesham Place, July 11, 1853.

Dear Sir,

It is with great diffidence that I venture to question the expediency of any of the resolutions adopted by my colleagues in a committee so constituted as that which I have the honour of addressing through you; I am well-aware that on many of the points referred to their consideration my opinion is entitled to little weight, but inasmuch as certain suggestions, which I had the honour to make verbally on other branches of the subject with which I am somewhat more familiar, seemed to some of my coadjutors entitled at least to be placed on record, I will without further apology venture to state very shortly my reasons for wishing that the Committee had not come to a definite resolution, that the speculum of the Southern Telescope should not exceed 4 feet in diameter. I assume the truth of the following propositions:—

1st. That it is the duty of the Committee to recommend to the Government the construction of such an instrument as in their judgment is best calculated to effect the object sought to be attained, regardless of the expense.

2ndly. That the object to be attained is, that the Southern Nebulæ should be subjected to examination by instruments as powerful as that by which Lord Rosse has lately so materially advanced our knowledge of the Northern.

The last proposition may be questioned by some; but it appears to me, that a speedy elimination of the laws which govern these remote and mysterious systems, is an inquiry well-worthy of the attention of philosophers, and that its success may greatly depend upon our taking immediate steps for handing down to posterity the forms and other peculiarities of these Southern Nebulæ by directing upon them a telescope possessing the same command of light, and all the other advantages which the genius and perseverance of Lord Rosse have brought to bear on the Northern. I can well believe that uniformity in the means employed in both hemispheres may exercise an important influence on the result. Assuming then the truth of the above propositions, the speculum of the Southern Telescope should be 6 feet in diameter: the objections to this size are,—

1st. The enormous cost of the original construction.

2nd. The increased expense of maintaining the telescope in working order; inasmuch as a larger proportion of copper must be used in the casting of the large speculum, and this will render it more liable to become tarnished, and necessitate more frequent repolishing.

3rd. The difficulties of mounting so large an instrument.

As to the first objection, I have already stated that in my opinion it ought not to induce the Committee to withhold from the Government that information, which they are most competent to give, as to the best mode of accomplishing a great scientific object. As to the second, the same principle applies; but I would remark that if the Government were zealously to take the matter up, and authorize

experiments to be made, and the ingenuity of our artizans were taxed and encouraged to provide an instrument of the magnitude in question, it is by no means certain that the difficulty might not be surmounted, and a composition discovered as little liable to tarnish as the smaller specula. As to the third objection, we have Lord Rosse's mounting to fall back upon; and ingenious as it is, who can say that it may not be improved, if proper encouragement be held out to makers possessed of zeal, leisure and extended experience in their art? On these grounds I am of opinion that either the 6-foot speculum alone, instead of the 4-foot, should be recommended with the proper estimates of cost, or that the recommendation should be framed in the alternative, accompanied by detailed estimates for the two sizes.

I remain, dear Sir, yours truly,

T. Bell, Esq. WROTTESLEY.

AT a Meeting of the Committee on the Southern Telescope, at the Royal Society, on July 5, 1853, The Earl of Rosse, P.R.S., in the Chair, it was Resolved:—

1. That the Committee approve the proposition made by Mr. Grubb, contained in Dr. Robinson's letter of the 30th of June.

2. That application be made to H.M. Government for the necessary funds.

3. That the Presidents of the Royal Society and of the British Association, accompanied by Dr. Robinson, who was associated with the Earl of Rosse in the former application, and Mr. Hopkins, President Elect of the British Association, be a Deputation to communicate with Government respecting the two preceding resolutions.

4. That the Earl of Rosse, Dr. Robinson, Mr. De la Rue and Mr. Lassell be a Sub-Committee for the purpose of superintending the progress of Mr. Grubb's undertaking.

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THE ROYAL SOCIETY.

SUPPLEMENT TO THE CORRESPONDENCE

OF

THE SOUTHERN TELESCOPE COMMITTEE.

[Printed by order of the President.]

Dr. Robinson to Col. Sabine.

July 28, 1853.

The copy of the telescope correspondence which you have sent me contains four letters which I had not previously seen, those of Mr. Cooper, Mr. Nasmyth, Mr. Lassell, and Lord Wrottesley. On each of these I wish to make some remarks.

1. If Mr. Cooper had stated that the *probable* value would be of an achromatic of 30 inches (£9000 for *the object-glass alone*), it would have decided the question, even without considering the exceeding great improbability of such an object-glass being obtainable on any terms. I will add, that I have reason to believe that there is not existing an object-glass of 12 inches and upwards *perfectly homogeneous*. Nor can I entirely admit that an achromatic is necessarily more permanent than a reflector. Professor Moll told me that a 6-inch Fraunhofer was more tarnished than a 9-inch Newtonian of Sir W. Herschel, both being in the Observatory of Utrecht. Besides Mr. Lassell's experience, I can state that a Munich prism and a 7-inch achromatic in my possession are both tarnished, though most carefully covered up.

2. Mr. Nasmyth's second mounting is very much better than his first one, but still has the defect of making the polar axis too long. The method of making the principal bearing of the polar axis run on friction-wheels, I *know* will not answer. It will produce what Mr. Sheepshanks once called "floating motion." Troughton's beautiful Equatorial at Armagh has it; that of my 15-inch Cassegrain, which was constructed by Mr. Grubb as an experimental model of Mr. Cooper's, had it till the friction-wheels were removed, Y's substituted, and counterpoise levers applied. In Mr. Cooper's this fact can be

verified any time by increasing the action of the antifriction sector till the axis rests on *it* alone. I may add that Mr. Nasmyth makes no provision for counterpoises, which however would be found essential.

As to the suspension of the observer, I dislike it very much, for this out of many reasons: the observer and his box must weigh at least three cwt.; this must produce deflection of the tube, different according to the zenith distance, and therefore making the adjustment of the mirrors vary with that element. An amount of displacement of the small mirror, much less than would occur here unless the upper part of the tube be very strong, will quite vitiate the instrument's performance.

The dew cannot in any case fall *on the speculum*, except near the zenith: but I do not see why it should *fall*; I think it more likely to trickle down the bars of the framework. Bibulous cloth would, I think, stop the air's escape, but wire gauze may be substituted, if such means prove necessary.

3. Mr. Lassell's suggestion of Malta would, I think, be attended with no advantage. The conditions at Hobarton must be nearly similar; and the expense of transporting the telescope there not very much greater.

The skeleton tube will have nothing to do except supporting the small mirror, and will certainly be much lighter than a tube of such sheet iron as must intervene between the great speculum and declination axis.

The composition of the metal *is* most important, but Lord Rosse's arrangements for that purpose are *perfect*.

A dome for this telescope must be nearly 60 feet in diameter, and I suspect would be a construction of great difficulty and expense. If the Cassegrain form be used, the observer can, as in Mr. Cooper's instrument, be sufficiently protected by a surrounding screen. The action of the wind on the skeleton part of the tube will be insignificant. I must also repeat what I formerly stated, that, as a *general rule*, telescopes act better in the open air than under a dome.

I can scarcely assent to the opinion that *doubling* the dimensions of Mr. Lassell's Equatorial would give sufficient strength for a 4-feet speculum. The strength of similar constructions is *inversely as their bulk*; the proposed one would therefore be relatively only half as strong, and the reduction of the standards would be very far from compensating this.

Kater long since thought that the Cassegrain had more light than the Gregorian. Tully refuted this by constructing a pair of these and a Newtonian of 6 inches. He found they had *exactly the same light*, but that the large mirror of the Cassegrain required a different figure from either of the others. In my 15-inch, Mr. Grubb made the large mirror parabolic and ground the small mirror to match it. I can use it either as Newtonian or Cassegrain, and am certain that the Cassegrain shows minute points,—for instance, the sixth star of the trapezium in Orion's Nebula,—better than the other; I am therefore disposed to think that Mr. Lassell had chanced

to match the Gregorian better than the Cassegrain. My small mirrors, I must remark, were ground on a miniature of Lord Rosse's polishing machine, of course with very different proportion of stroke.

I do not imagine Mr. Lassell's levers, of which I think very highly, at all require the condition that one diameter of the mirror shall be always in the same vertical plane. In the arrangement of those which I have designed for a large reflector (which I hope to construct), the objection is met by giving them *spherical* bearings in the system of levers used by Lord Rosse, which will also reduce them into a very small compass.

4. Lord Wrottesley's opinion in regard to a 6-feet telescope is certainly entitled to great weight, and it is not without hesitation that I would suggest my doubts. I think we cannot value the cost as less than in the ratio of the cube of the aperture, or about £15,000. This I fear we have no chance of obtaining.

Lord Rosse's two specula are both *by accident a trifle below the true standard.* I do not attach any importance to this; but still cannot speak with the same decided confidence as to the casting them in the fine alloy as I would of the smaller size.

The difficulty in using a reflector caused by the state of the air is as the area of the mirror in any case, and (in a close tube) as the depth of air in the tube, all which is eddying about. The skeleton tube will, I hope, diminish this last, but still we can scarcely expect the proportion to be less in the six and the four than $2\frac{1}{2} : 1$. How far this may be influential in the finer climate, where I hope this instrument will be established, I am unable to say; but in Ireland I am sure that there are three times as many nights when a 3-feet can be used with full advantage as a 6-feet.

At the same time I see no objection to giving Government the option, and I have requested Mr. Grubb to furnish me with an estimate (which of course will be less detailed) for a 6-feet instrument. He sees no difficulties except the far greater uncertainty of casting and polishing, and the increased expense.

I have perhaps given you unnecessary trouble in this long letter, but it may not be useless to meet any objections that might arise from the points which I have considered being supposed to pass unanswered. Yours ever,

T. R. Robinson.

To Col. Sabine.

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THE ROYAL SOCIETY.

CORRESPONDENCE

REGARDING

THE MELBOURNE TELESCOPE.

CORRESPONDENCE.

I. *General Sabine to William Lassell, Esq.*

Burlington House, London,
October 20, 1862.

MY DEAR SIR,

I enclose a copy of a despatch from the Governor of Victoria to the Duke of Newcastle respecting the erection at Melbourne of a telescope of greater power than any previously used in the Southern Hemisphere; the despatch, with its accompanying papers, have been sent by His Grace to the Royal Society, with a request that the President and Council will furnish him with a report upon the subject.

You will not have forgotten the correspondence which took place in 1852 and 1853 upon the proposition which (being then President of the British Association) I had the honour of making to the President and Council of the Royal Society, for their cooperation in representing to Her Majesty's Government the importance of establishing a telescope of great optical power in a convenient locality in the Southern Hemisphere. Your own letters formed a very important part of that correspondence; and as you may not have copies of them with you, I send by the same post which conveys this letter, a printed copy of the whole correspondence.

The Duke of Newcastle's communication will be laid before the Council at its first Meeting after the recess (October 24th). We may confidently anticipate that the Council will entertain the same opinion as that of the Royal Society and British Association at the former period, in respect to the importance of the objects to be obtained by the employment of a telescope of large power in the locality of Melbourne, and that they will also highly appreciate the distinction which the Colony of Victoria will acquire by this act of its Government and Legislature. But you will perceive, by the concluding sentence of the Minutes of the Board of Visitors of the Melbourne

Observatory, it is requested that the Report solicited from the Royal Society may contain "an expression of opinion as to the most suitable construction of telescope for the purpose, both as to the optical part and to the mounting, together with its probable cost, and the time requisite for its completion." Now these are precisely the points which appear to have occupied the chief attention of those who participated in the correspondence of 1852 and 1853; and it is probable that the resolutions which were agreed to at the final Meeting of the "Committee on the Southern Telescope" on July 5, 1853 (page 45 of the printed correspondence), may be regarded by the Council as embodying substantially the conclusions arrived at after such long and patient consideration. The summary of these appears to have been that the telescope to be recommended should be a 4-feet reflector with mounting, &c., as recommended in Dr. Robinson's letter of June 30, 1853; and that it should be made by Mr. Grubb, under the superintendence of a Committee consisting of the Earl of Rosse, Dr. Robinson, Mr. De la Rue, and Mr. Lassell. Mr. Grubb's estimate of the probable cost of the Telescope, two Specula, a polishing Machine, and a one-horse power Steam-engine was £5000; but as several things in the construction were necessarily experimental, it was put at £6000,—Mr. Grubb's estimate of the time requisite for its completion being a twelvemonth from the date of commencement.

The Council will probably desire in the first instance to learn whether any modifications have taken place in the opinions as to the telescope, &c. then entertained by the four gentlemen who were willing to undertake the responsibility of superintendence. As you are at a distance, and time is important, I have thought it worth while to write in anticipation of such more official communications as may be written after the Council has met. You will probably hear from me again by the next mail; but in the mean time Dr. Robinson has requested me to ask you if you have with you an estimate of the cost of your 4-feet Newtonian with its mounting.

Believe me, my dear Sir,
Faithfully yours,
EDWARD SABINE.

William Lassell, Esq.

P.S. It may be desirable that the person selected to use the telescope at Melbourne (who, I presume, will be Professor Wilson) should be in this country during a part at least of the time in which its construction should be in progress, and that he should avail himself of such opportunities as may be afforded of making himself thoroughly conversant with the methods, &c. adopted at your own and Lord Rosse's observatories.

Communications referred to and enclosed in the preceding Letter, No. I.

Downing Street, 16th October, 1852.

SIR,

I am directed by the Duke of Newcastle to transmit to you, to be laid before the President and Council of the Royal Society, a copy of

a despatch from the Governor of Victoria, respecting the erection in Melbourne of a telescope of greater power than any previously used in the Southern Hemisphere.

It will be seen from this despatch that Sir H. Barkly is desirous of obtaining the cooperation of the Royal Society towards effecting this object, and that he would be glad if they would furnish a report upon this subject.

His Grace feels no doubt but that the Royal Society will readily do whatever may be in their power for encouraging science in the Colony of Victoria.

I am, Sir,
Your obedient Servant,
T. F. Elliot.

The Secretary of the Royal Society.

Governor Sir H. Barkly to the Duke of Newcastle.

(Copy.)

Government Offices, Melbourne,
23rd July, 1862.

My Lord Duke,

The Board of Visitors to the Melbourne Observatory, over which I have the honour to preside, being of opinion that the project long entertained, of erecting in the Southern Hemisphere a telescope of much greater optical power than that used by Sir John Herschel at the Cape of Good Hope, would be materially advanced by an expression of interest and sympathy on the part of scientific men in England, has requested me to bring the subject under Your Grace's notice, with a view to its being submitted for the Report of the Royal Society of London and the British Association for the Advancement of Science.

I have great pleasure in forwarding accordingly, with the approval of my advisers, an extract from the Board's Minutes, together with the accompanying letter from its Honorary Secretary, Professor Wilson, in which the reasons for this step are so fully set forth, and the advantages likely to arise from obtaining a powerful instrument for this purpose so clearly explained, as to leave nothing for me to add beyond earnestly soliciting Your Grace's good offices in the matter.

It will be observed that the pecuniary cooperation of the British Government is not applied for; but I need hardly say that even the smallest donation from that quarter would much facilitate raising the necessary funds.

I avail myself of this opportunity to put Your Grace in possession of the Second Annual Report of the Board of Visitors, from which it will be found that a commencement has been made in the erection of the new Observatory, advocated in the Report previously transmitted; and I am glad to be able further to state that a sum of £4500 has since been voted by the Legislature for the completion of the requisite buildings.

Should it be possible therefore to add an equatorially mounted telescope, the Astronomical Branch of the Observatory will be rendered complete, and no greater expense than at present will be incurred for the Staff attached to it.

I have, &c.,
(Signed) HENRY BARKLY.

His Grace the Duke of Newcastle, K.G.,
&c. &c. &c.

Professor Wilson to Sir H. Barkly.

(Copy.)

The University, Melbourne,
16th July, 1862.

SIR,

I have the honour, by direction of the Board of Visitors to the Observatories, to forward to Your Excellency the accompanying extract from the Minutes of a Meeting held yesterday, and to express a hope that you will comply with the request contained in it.

Though entertaining no doubt of the importance of the results to be obtained by such a telescope as is recommended, or of the conspicuous and creditable position which Melbourne would consequently occupy in the eyes of all persons in Europe who take an interest in Science, the Board is desirous of obtaining an expression of opinion from scientific men in England, because it is due to those who may be asked to contribute towards its accomplishment that the importance of the object should be attested by higher scientific authority than the Board can lay claim to; because also it considers that every means should be used to obtain, so far as funds will permit, the best instrument which modern skill and recent inventions render possible; because, finally, the Board feel that, whether the cost of the instrument be defrayed wholly or partially by private contributions or a grant from the Legislature, public sympathy will be much more strongly enlisted in its favour by a statement of the interest taken in the matter in Europe, and by the approval of the Imperial Government, than by any representation which the Board can make.

I have, &c.,
(Signed) W. P. WILSON,
Secretary to the Board of Visitors.

His Excellency the Governor.

Extract from the Minutes of a Meeting of the Board of Visitors to the Observatories, held 15 *July* 1862.

"The attention of the Board having been drawn to the following circumstances:—

"I. That, as long since as 1849 the facts brought to light by

Lord Rosse's Telescope were judged by the Royal Society of London and the British Association for the Advancement of Science to be so important as to justify them in making an urgent appeal to the British Government for the erection, at some suitable place in south latitude, of a telescope for the examination of the multiple stars and the nebulæ of the Southern Hemisphere, having greater optical power than that used by Sir John Herschel at the Cape of Good Hope; which appeal there is little doubt would have been successful but for the Russian war and the consequent expenditure;

"II. That, since that time, Lord Rosse reports that he has discovered systematic changes in some of the most important northern nebulæ;

"III. That the interest and scientific importance of the solution of the problem of their physical structure, as well as the probability of its accomplishment, is thus greatly increased;

"IV. That some of the most important nebulæ, and those presenting the greatest variety of physical features in close proximity, can be observed only in places having a considerable southern latitude;

"V. That the geographical position and clear atmosphere of Melbourne render it peculiarly suitable for this work, and that the arrangements already made for the establishment of an Astronomical Observatory on a permanent footing offer great facilities for carrying it on;

"VI. That, independently of the especial object to which such telescope would be applied, an Astronomical Observatory cannot be considered complete without an equatorially mounted telescope of large optical powers:

"It was Resolved,—

"1st. That, in the opinion of the Board, the establishment of such a telescope in Melbourne would materially promote the advancement of science.

"2nd. That, before applying to the Colonial Government for any pecuniary grant in aid of this object, His Excellency the Governor be requested to obtain, through the Secretary of State for the Colonies, an expression of opinion from scientific men in England as to the importance of the results to be expected from it; the most suitable construction of telescope for the purpose, both as to the optical part and the mounting; its probable cost, and the time requisite for its completion."

II. *William Lassell, Esq., to General Sabine, R.A.*

Malta, Nov. 5, 1862.

My dear Sir,

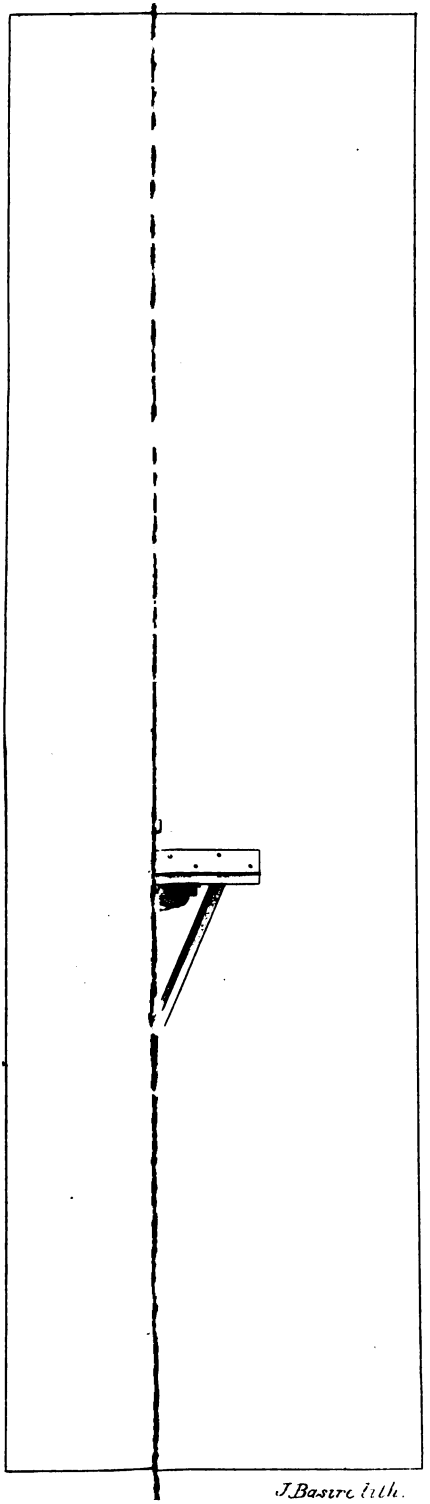
Your letters of the 20th and 23rd ult. reached me by the same mail on the 1st inst., and from the former having an extra stamp on more than the weight demanded, I presume it must have been intended to come by the previous mail; it bears the London Post Mark

of the 22nd. The printed copy of the correspondence on the Southern Telescope in 1852 and 1853, which you mentioned having forwarded to me, I have not yet received. [It has arrived this evening.]

It appears to me, however, that the question of the construction of a four-foot equatoreal has very much altered in its bearings since the date of that correspondence, inasmuch as the problem has been completely solved by the erection of an equatoreal precisely of that size and description in this island. I enter upon the subject with diffidence, because I may appear to speak too much of myself, though I may think that good faith and the necessity of the case make it inevitable. I believe that the telescope I have erected is more efficient and powerful, and combines more fully the desired qualities of a large telescope, than any which has yet been made.

After a year's experience of its use, including some small additions to its apparatus which have been made here, I have not become aware of any material errors which have been committed in its construction, nor have any improvements of moment suggested themselves which would greatly add to its convenience. Some slight additions or variations I might suggest in the building of another similar telescope, but none I believe of very great importance. The construction of the levers for the prevention of flexure, renders it not desirable to observe more than about 15° north of the zenith, though the mounting is calculated to direct the telescope within a few degrees of the pole; and I have not as yet observed (at any hour-angle) within 35° of it, principally because the arrangement of the revolving tower does not admit of getting conveniently at the eye-piece in such positions. If required to observe there, some removeable outrigger to the tower would have to be contrived. I found it impossible to get access to the eyepiece near the pole and the southern meridian with equal convenience, and therefore sacrificed the former to the latter.

I send you by this mail a lithographic drawing of my equatoreal, should it be thought desirable to adopt its plan. (Plate I.) It is at least not an untried scheme, and as Melbourne is within 2° of the same latitude as Malta, scarcely any modification would be necessary. In answer to your inquiry about the cost, I may say that, including every expense which ought fairly to be charged, it does not quite reach 2500 pounds. And this includes steam-engine, grinding- and polishing-machine, casting apparatus, observing-tower, and circular railway, erecting and dismounting in Liverpool, and re-erecting here. If I were in Liverpool the same thing could of course be done again, and for the same sum; and I should be almost tempted (if it were desired) to offer my superintendence as a pure labour of love. That, however, being out of the question, I should be glad to furnish any information I possess upon the subject, and to place my plans and drawings at the service (for copying) of any gentleman who may be appointed to superintend and carry out the work. And here I venture to repeat the opinion expressed in my letter at page 17 of the "Correspondence," that the work must be executed entirely under the direction of a single individual. A committee may



J.Bastre lith.

determine generally upon the *plan*, but the execution I am firmly persuaded must be confided to one alone. Messrs. George Forester & Co., of Vauxhall Foundry, constructed almost entirely the mounting, and are, I doubt not, still in possession of the working drawings made from my original sketches. The work is of the most substantial and accurate character, fit to endure with reasonable care for ages.

I believe I may speak with confidence of the very great efficiency of the mounting for the most delicate work, such as measuring in position and distance the faint satellites of Uranus, the satellites of Neptune, and the faint neighbouring stars of planetary nebulæ, as well as measurements of the dimensions and forms of the latter bodies. The micrometer I use consists of thin, or rather narrow brass bars, moveable of course in position and distance. These I manage to illuminate without letting any sensible light into the field; and it is really a beautiful observation to see Neptune and his conspicuous satellites on the blackest field with a power of 1060, while there is abundant light on the bars to allow of their being placed in any required position with respect to the objects to be measured; the position and distance are read off at one observation. Of course to do this with all attainable accuracy, tests the driving motion; and I have attempted occasionally to follow a practice I had with the two-foot equatoreal, of making the contact more perfect and more enduring by a slight regulated pressure with the fingers upon the tube; but I find this instrument too stiff in $\mathcal{A}$R to allow me to do this without a painful amount of pressure; and therefore, when necessary, I have recourse to the "slipping piece." This disadvantage in one respect will be thought by some to be generally an advantage, and a very valuable one.

The various storys of the tower form each a snug chamber about 4 ft. 6 in. square, in which the observer is protected from the wind or dew, though close to the eyepiece, and has his writing or drawing materials on a fall-table close to his hand. The skeleton-tube is a great improvement upon the closed one, and I attribute in a great measure to this form, the fact that I am *very rarely* driven to close the observatory from atmospherical disturbance; though of course with so large a telescope and with high powers, *some* vibration is always perceptible. I feared that this open form of tube would have subjected me to drops of dew falling from the bars upon the mirror, and had contrived a means of obviating it: but the annoyance does not exist. On not more than two or three of the most dewy nights have I detected some half dozen drops on covering up the mirror. I may add that if they should remain unwiped they leave no stain; the alloy is so pure that I have as yet detected no indication of tarnish whatever.

It is therefore by no means necessary to repolish frequently, though there is no reason for hesitation on the ground of risk; for with the usual amount of care in the operation, a good and efficient surface, at least, may be always depended on. The process when resorted to consumes a day, and the operation of taking out one speculum and

putting in another a like period. I may be observing today until midnight, or until dawn with A, and before sunset tomorrow B shall be in its place and ready for observation—the work of two individuals.

With respect to permanence of adjustment, I have never touched the polar-axis screw since it was first set up, nor have any observations sufficed to indicate any sensible error. The inevitable labour of turning so large a telescope about from 3 or 4 hours angle on different sides of the meridian and at various polar distances, with the apparent inutility of such an operation, has prevented my undertaking it, since I do not pretend to give absolute places, since I have no difficulty in finding any objects I want at once, and since when I want especial accuracy, a star from some catalogue and the micrometer enable me to obtain it. I believe the defining power is quite equal to that of any two-foot equatoreal, which is more than I expected originally to obtain. *How* well it defines it is difficult to express in words, *that* can only be known by personal examination; or perhaps it may in some degree be inferred from the series of drawings of the phenomena of the surface of Mars which I have made during the late opposition. I acknowledge, however, that I have not been able to do any justice to the number and variety and delicacy of the shades and tracery of his disk. The best magnifying power on fine nights was usually 760.

The full time occupied in the construction of my equatoreal was between three and four years; and although much less divided attention might be given to the erection of another than I was able to afford, I do not think it could be accomplished in less than 18 months, even if all went well and much diligence were used. In Mr. Grubb's estimate, page 37 of the "Correspondence," is the status of the observer included, and the machinery necessary for placing him conveniently at the eyepiece in all required positions of the instrument? this is a rather formidable part of the work. Does it also include driving motion, and cost of erecting for examination and taking down?

I feel that I need to apologise for the length of this letter, from which I fear you will conclude that I have thought it better to say too much than too little.

I have indeed thought it desirable to make the Committee as fully acquainted as I can in a letter with the qualities of the instrument I have put up, and have only to add that if they think it worth while to depute any competent gentleman to come over and acquaint himself with the construction, use, and optical powers of this telescope, I shall be glad to afford him the opportunity of the fullest examination, and to place what information I possess upon the subject at his disposal.

Believe me to remain,
My dear Sir,
Very truly yours,
WILLIAM LASSELL.

To General Sabine, R.A.,
President of the Royal Society.

III. *The Rev. T. R. Robinson to General Sabine, Pres. R.S.*

October 23, 1862.

The conviction of the importance of a searching examination of the Southern Nebulæ with an instrument of great optical power, which I expressed in my letter to Lord J. Russell (Brit. Assoc. Report, 1850), has been strengthened by the facts recently observed by D'Arrest and others in the northern hemisphere, respecting the variable character of some of these strange forms, and by Lord Rosse's observations lately communicated to the Royal Society, which indicate in a few considerable proper motion and even physical changes. It is evident that in the investigation of such variations *time* is a most important element; and future astronomers will look back with unavailing regret to the present opportunity of leaving them so precious a record of half the sky if it be permitted to pass away without results. As to the means of carrying out such a survey, I am not aware that much can be added to what is contained in the printed correspondence. Since its conclusion, Mr. Lassell has successfully constructed a Newtonian of the same aperture (4 feet) as that proposed for the southern telescope. This shows that there is no special difficulty in such an enterprise, either as to the telescope or its equatorial mounting; but I retain my original preference for the Cassegrain as far more manageable, and, I may add, better adapted to photography. The application of silver surfaces to form specula which has been made by M. Foucault, seems very promising, but has not yet been sufficiently tested by experience for me to recommend its trial in a case where failure would not only interrupt our hopes of a great astronomical achievement, but would also probably weaken the efforts of those who are endeavouring to diffuse a taste for science in this prosperous and energetic colony. There seems a tinge of exaggeration in the account of the telescope which M. Chacornac is trying that inspires doubt; and some uncertainty prevails as to the actual reflective power of the silver film. Nor have we sufficient knowledge whether the process which availed to figure a *glass* disk of 33 inches would be equally successful for one of 48 inches; or whether such a disk might not be more affected in its curvatures by changes of temperature than one of better conducting-power. The Foucault system could easily be adapted to the small mirror; or in the Cassegrain (as has been tried successfully) this might be an achromatic combination silvered at its posterior surface. But as to the great speculum, I think the usual material is safest at present.

T. R. Robinson.

General Sabine, Pres. R.S.

IV. *Sir J. F. W. Herschel to General Sabine.*

Collingwood, October 29, 1862.

My dear General Sabine,

I have received and carefully read the copy of the correspondence of the present year in the matter of the great reflector for the Mel-

bourne Observatory, and the extracts from the Minutes of the Meeting of the Colonial Board of Visitors of Observatories accompanying your letter. In reply to the request contained in your letter, that I should offer such remarks on the subject as may occur to me, I beg to say that my opinion as to the desirableness of such an instrument at that or some other equally well-situated station in the south hemisphere has been of late materially strengthened by the instances of change in the brightness and general appearance of nebulæ in the northern hemisphere, which have of late been brought forward, and which, in two cases at least, appear to be undeniably authenticated (that of the Nebula M. 80, and the last Nebula of Hind). To these instances I am enabled to add another and, if possible, more striking example of change which has manifested itself in the great southern Nebula surrounding η Argûs. In a letter which I have lately received from Mr. Eyre B. Powell, late Principal of Madras College, (himself a good observer, and who has contributed very valuable observations of double stars and computations of their orbits, and whose statement of what he himself has observed during a continued series of micrometrical measures of the stars around η Argûs may therefore, I conceive, be fully relied on,) he expresses himself to this effect.

"One point I consider particularly worthy of attention, viz. that while in your diagram of the Nebula the southern end of the curious oval vacuity is clearly bounded, it is now decidedly open."

This oval, with its perfectly well marked and *most completely closed* southern boundary, I was at peculiar pains to delineate with the utmost precision, and the course of that boundary is marked by the positions of small stars in close proximity to its edge most carefully laid down (*vide* my account and figure of this nebula in my Cape Observations); and as it seems impossible to cast any doubt on so direct and circumstantial a statement as that of Mr. Powell, we have here an instance of change which must be regarded as perfectly reliable; nor can I imagine anything more important to our knowledge of these wonderful bodies than to obtain as early as possible exact delineations of the present state of this, and, of course, of all the other irregularly-shaped nebulæ of the Southern Hemisphere, by the aid of a very powerful reflector.

As regards the general form and construction of the telescope—referring to my former letter of March 1853, printed in the "Correspondence of the Southern Telescope Committee,"—I should continue to insist on the following points there specified, whatever be the construction and mode of mounting, viz.:—1st. The use of a very perfect internal collimator. 2ndly. The adoption of a skeleton tube. As regards the uniform support of the mirror, since it appears to be now a received maxim in the construction of large specula that they must be polished *in water*, and lying on their backs on the actual system of support which is to sustain them in the tube, this of course puts an end to the idea of supporting them on beds of any elastic material other than metallic, and the rusting of steel springs would be fatal to *their* substitution. But the principle of swing suspension, by

a band round the edge to take off the distorting pressure of the vertical component of the weight on a point or on a small portion of the edge, I still regard as most important and indispensable.

Dr. Robinson, I observe in the letter sent me (which I reinclose), continues to recommend the Cassegrainian construction. Its great advantage in shortening the telescope I fully admit, but I continue to regard this as much counterbalanced by the excessive distress caused to the observer who shall have to continue observing for many successive hours with his head elevated to a great angle with the horizon, and in many cases to recline on an observing-chair. I speak from feeling experience of this inconvenience (as contrasted with the comfort of the Newtonian or the front view), and I am persuaded it will act to shorten the hours of observation from mere physical inability to go on. But I agree with Dr. Robinson in deprecating the hazard of an experiment on so great a scale as the construction of a silvered glass large mirror on the principle of Steinheil and Foucault.

The only reason which would greatly weigh with me in favour of an equatorial in preference to a meridian mounting is the immense advantage such a telescope would afford for examining the physical construction of comets. In this class of observations, as in that of nebulæ, increase of illumination is the paramount consideration. The difference of appearance of one and the same comet under telescopes of high and low illuminating power must be seen to be duly appreciated. Mr. Lassell appears to find his recently constructed equatorial mounting work satisfactorily, but on this point he will no doubt communicate the result of his own experience.

Finally, if the aperture 4-feet be not already held to be irrevocably fixed upon, I should feel half inclined to open the question of a 5-feet for the reconsideration of the Committee.

I remain,
My dear General,
Yours very truly,
J. F. W. Herschel.

P.S. Nov. 19, 1862.—Since writing the above I have been favoured with the perusal of Mr. Lassell's letter, dated Nov. 5, 1862 (and herewith reinclosed). By this I find, 1st, that his system of levers for the prevention of flexure in the mirror does not admit of, or at all events renders it undesirable to observe more than about 15° north, *i. e.*, to the polar side of the zenith; and 2ndly, that the construction of his tower does not admit of getting conveniently at the eyepiece within 35° of the pole.

Now these conditions would I fear be fatal to the use of the instrument at Melbourne, for the observation of the southern nebulæ. The *immediate* neighbourhood of the South Pole, indeed, is not rich in objects of interest. There is, to be sure, a faint nebula within half a degree of the pole (h. 3176), but from thence to S.P.D. 9° 59′ (say 10°) (where there is a fine planetary nebula, h. 3241), there occur only five nebulæ, all of little interest. This region then might be sacrificed without material injury. But the two nubeculæ are

objects of paramount importance and interest. The nubecula major extends from S.P.D. 18° to 24°, or fron zenith dist. 28° to 34°, and the nubecula minor from S.P.D. 15° to 18°, zenith dist. 34° to 37°. The great nebula about η Argus itself is only 31° 13′ from the pole, and passes above the pole at Z.D. 20° 47′, taking the latitude of Melbourne at 38° S. Unless therefore Mr. Lassell's construction can be so very considerably modified as to allow these objects at least to be observed with every possible advantage, I fear it must be abandoned. Indeed I should consider perfect facility of observing up to 10° S.P.D. or Z.D. 42° south quite indispensable.

J. F. W. Herschel.

V. *The Earl of Rosse to General Sabine.*

Dublin, October 30, 1862.

Dear Sabine,

I have not received the correspondence from Mr. White, but I sufficiently recollect the purport of it. The only change I am aware of, which has since taken place, is the construction by Mr. Lassell of a telescope similar in aperture, and very similar in mounting to the one recommended.

If Mr. Lassell is *quite satisfied* with his mounting, I do not see why an exact fac-simile of it should not be made. There could not be anything cheaper, and although I do not think differential observations beyond the range of the micrometer could be obtained with much accuracy with it, as the position of the polar axis is maintained by rollers, still for work upon the nebulæ it would probably answer sufficiently well. Upon this point, as also upon the question of steadiness, if there was time, it might be well to consult Mr. Lassell. Mr. Lassell, no doubt, as we do, has kept his patterns, and, I am sure, would be most happy to lend them. That would save expense.

I think it essential that some one should accompany the telescope as a mechanical assistant—*he having been the principal operator in grinding and polishing the specula.* Some intelligent optical glass-grinder who was sufficiently educated to read and understand all that had been written on the construction of reflecting telescopes, would probably answer.

Believe me to be,
Truly yours,
Rosse.

General Sabine.

VI. *The Earl of Rosse to General Sabine.*

The Castle, Parsonstown,
November 9th, 1862.

Dear Sabine,

I have read Herschel's letter with much interest, and agree with him in everything. The only objection to the increase of size he suggests is the expense, which probably would be more than doubled.

Believe me to be,
Truly yours,
Rosse.

General Sabine.

VII. *The Rev. T. R. Robinson to Major-General Sabine.*

Castle, Parsonstown, Dec. 4, 1862.

My dear Friend,

Since my letter to you on the Southern Telescope, I have been favoured by your kindness with a perusal of letters on the same subject from Lord Rosse, Sir J. Herschel, and Mr. Lassell.

Mr. Lassell's communication is very interesting, as describing his success in constructing and mounting equatorially a four-foot telescope, which seems a very good one, and with which he is undoubtedly doing good work. But with the conclusion to which he seems to come, that this construction is the best possible, and should be implicitly adopted, I am not disposed to agree.

It seems to me, if I rightly understand your wishes, that it is not our province to decide on any particular construction, but in the first instance to supply the Council with an expression of our general opinion as to the feasibility and great importance of a Southern Reflector; and secondly, to give whatever practical information we have acquired since the former Correspondence. This the Council may, if they think it necessary, make the subject of a separate communication to the Melbourne Astronomer; but it is, I think, highly inexpedient to bring before *public men* our discussions as to the best mode of doing the work.

In reference to the first of these objects, I think the letters contain all that can be required; but with respect to the other, Mr. Lassell's letter seems to be liable to several objections, based on some facts which he mentions, and also on the lithograph which accompanies it.

1. The upright standards between which the telescope turns, are weak. Mr. Lassell has felt this; for there appears a transverse trussing marked "removeable," obviously designed to prevent flexure when the telescope is a few hours off the meridian: this I fear would be an imperfect remedy. I cannot ascertain from the drawing whether the declination-axis is provided with counterpoises and friction-wheels to diminish its friction; but if not, it will bear very severely on the flanches of this axis, especially in the position of 6 hours off the meridian. The flexion of these standards may interfere greatly with the adjustments of the instrument, if they be not of equal strength.

2. The support of the polar axis on friction-rollers at its upper bearings is, as Lord Rosse has remarked, fatal to any precise determination of absolute places. Mr. Lassell appears to admit this, but not to think it of much importance, "as he can always find an object which he wants," and, "when greater accuracy is required, a star taken from a catalogue with the micrometer will give it." On this I must remark that, in the first place, it is not always easy to identify the stars of comparison, and that in nebular work there will be a great waste of time unless the instrument can give position within a small fraction of a minute. Mr. Lassell seems in fact not to have taken much trouble in verifying his adjustments, and explains this by "the inevitable labour of turning so large an instrument about

from 3 to 4 hours' angle on different sides of the meridian and at various polar distances." It may be inferred from this that there *is* considerable difficulty in moving it. He has stated in the Monthly Notices of the Astronomical Society that the moving parts weigh 12 tons—a large mass certainly, but I think that with proper mechanical appliances its motion should not require such hard work. The counterpoise on the polar axis must make the movement in AR more difficult.

3. The tower seems ricketty and even dangerous. There may be guards for the observer not shown in the lithograph, but even with them his position in a dark night will be perilous. And as Mr. Lassell mentions that with this arrangement he cannot observe within 35° of the pole at any hour-angle, Sir John Herschel's remarks are decisive against it. If, in addition to "the labour of moving the telescope," the observers have to manœuvre this complicated scaffolding, the most probable result is that it will scarcely ever be used except near the meridian.

4. Mr. Lassell's estimate of £2500 for such an instrument is, I fear, lower than any optician is likely to accept. I understand it to include the telescope, equatoreal, tower, and its railways, steam-engine, machine for polishing and grinding, grinder and polisher, with two specula, besides the cost of erecting, dismounting, and re-erecting. If the specula be of the same proportion as Lord Rosse's, they will each be 2 tons; their raw material will cost £400; and as a commercial speculation, I do not think any one would undertake the risks of casting, annealing, grinding, and polishing for less than £1000 more, which leaves a very small margin for the rest. But this concerns the Melbourne gentlemen, who will of course obtain detailed estimates.

5. The report of the action of the skeleton tube confirms Lord Rosse's results from one which he had applied to his three-foot telescope. I send a photograph of this latter, which I consider decidedly preferable from its great mechanical strength and large airway.

6. There appears to be some difficulty about the levers which equilibrate the mirror in the direction of its plane, so that they do not act rightly more than 15° on the polar side of the zenith. This can probably be obviated; but I am induced by my own experience (certainly on a much smaller scale) to think that the plan of supporting the edge of the mirror in a flexible metallic ring will be quite sufficient even for one of four feet, and that *it* should be tried before recourse is made to the more complex arrangement. As to the back-support, we know that the equilibrated levers used by Lord Rosse work very well; but I cannot help speculating on an elastic bedding such as worked so well with Sir J. Herschel, and recently with M. Foucault. As it must be water-proof, we are limited to some structure of metal combined with caoutchouc, or to a piston sliding in an air-tight cylinder. For a six-foot speculum weighing 4 tons, the pressure on each square inch of such a support is only 2·2 lbs.

7. Mr. Lassell asks if Mr. Grubb's estimate includes a driving-clock, and if he has provided for the status of the observer. The

clock is included, and also a differential hour-circle. As to the other, since the eye-end of the telescope (Cassegrain) travels in a sphere of only six-feet radius, an observing chair of simple construction and moderate size is amply sufficient. Lord Rosse has remarked that a skilled mechanic should take part in the polishing, and afterwards accompany the telescope when erected at Melbourne. In the principle I fully concur; but perhaps it might be better if the Australian observer himself were to come over, and while the process was going on, make himself familiar with all its details. The polishing large specula is "skilled work" of the highest order, and I do not think it can be effectually superintended by a mere mechanic.

Sincerely yours,
T. R. Robinson.

Major-General Sabine,
President of the Royal Society.

VIII. *William Lassell, Esq., to General Sabine.*

Malta, Dec. 1, 1862.

My dear Sir,

I have today had the pleasure of receiving your letter of the 20th ultimo, enclosing some remarks of Sir John Herschel upon mine to you of the 5th. I avail myself of the Alexandrian mail now hourly expected for a few words in reply.

I fully appreciate Sir John's requirements of "perfect facility of observing up to 10° S.P.D., or Z.D. 42° S.," and proceed to state how, by a slight modification of my mounting, it would be completely satisfied.

By omitting the loose cross marked "removeable" in my drawing, and giving about a foot more length to the wrought-iron supports of the ends of the P.D. axis, the telescope would have a motion quite up to the pole itself; and to ensure the effective action of the levers for horizontal support, it would be only necessary, when the meridional zenith-distance towards the pole became greater than about 15°, to turn the telescope on its axis 180° by the existing apparatus; so that the eyepiece should point westward instead of eastward, and bring the tower round to that side. It would also be necessary to have a P.D. arc on the western as well as the eastern support, with tangent screw and locking apparatus, to which in this position of the telescope, the connecting-rod (being disengaged from its former position and carried round with the tube) would have now to be re-engaged. Thus the instrument by this slight addition would command equatorially every star above the horizon.

To enable the observer to gain access to the eyepiece when the object was near the pole, above or below, it would be necessary to introduce a third rail, the same distance from the outer one which

that is from the inner, and to make the lowest beams of the framework of the tower to travel outwards on two parallel rails extending from the inner rail to the new outer or third rail—thus allowing the framework of the tower to rest at pleasure either, as it now does, on the inner and what would then be the middle rail, or on the middle and new outer rail. This would ensure access to the eyepiece in every possible position of the telescope.

Though I have had a very short interval to consider the subject, I feel perfectly satisfied that the above easily-carried-out additions would quite answer the requirements insisted on by Sir John Herschel.

I remain,
My dear Sir,
Very truly yours,
General Sabine. Wm. Lassell.

IX. *The Earl of Rosse to General Sabine.*

The Castle, Parsonstown,
Dec. 3, 1862.

Dear Sabine,

Dr. Robinson has just shown me Mr. Lassell's letter of Nov. 5. I was not aware that his telescope would not reach the pole. Probably it would be sufficient could it be made to do so at moderate distances from the meridian. I am not sure whether I should not be disposed to recommend in the first instance a common cheap wooden stand like Herschel's, or perhaps one on the same principle, but in the more substantial form of our 3-foot telescope. With the lattice tube the wind acts but little upon it. Such an instrument has done good work in Herschel's hands, and with us it is constantly employed in making lunar drawings, the want of an equatorial motion being scarcely felt. In such a case it would be only necessary to send out the specula; and even that perhaps would not be necessary, as with a competent person I should think they might be made on the spot.

I am only suggesting the cheapest and easiest way of bringing great power to bear upon the objects in the Southern Hemisphere, and not for a moment proposing an inferior method of mounting, if the equatorial (the best) can be attained. As to the Cassegrain form, my experience is limited to specula of 18 inches' aperture, and very few experiments have been made with them; I am quite unable to say whether they define as well as Newtonians or not.

I have been reminded by Dr. Robinson that in my last paper I have omitted to mention a little expedient we have long been in the habit of resorting to, especially in polishing with simple pitch, that of raising the temperature of the room, and consequently of the polishing tool, above the temperature of the speculum. In this way the necessary yielding of the pitch is provided for, without softening the *surface in contact with the speculum*. When the process is not proceeding successfully, by thus raising the temperature of the room

a few degrees good contact will often be procured, and the disappointment of a failure prevented.

This hint may be valuable to persons who have not so much experience.

Believe me to be,
Truly yours,
General Sabine. Rosse.

X. *The Earl of Rosse to General Sabine.*

The Castle, Parsonstown,
Dec. 9, 1862.

Dear Sabine,

I have forwarded Lassell's letter of Dec. 1 to Dr. Robinson. I see no reason why Lassell's mounting, modified in the way he proposes, should not answer, provided the main supports, which are to be lengthened, are made sufficiently strong—much stronger than they are at present. They, in my opinion, constitute the weak part of the mounting. They are theoretically weak, and that they have been found weak in practice is evident, as otherwise the moveable bracing would not have been employed. The great merit of Lassell's mounting is its cheapness; for it is almost all the work of a founder and boiler-maker, and at Manchester and Liverpool there are special appliances which make such work cheap. In the case of the German type there is much forging, turning, and fitting, and therefore there is greater cost unavoidably; but where expense is not an object it has many advantages. Lassell, I understand, casts his specula at *one* melting, making use of a bell-founder's cupola, and probably is enabled to procure trained men, and every thing necessary (except the mould and annealing oven) at some large bell-founding establishment. In that there will necessarily be a great saving. Hitherto it has been considered important that there should be two meltings, and therefore to prevent oxidation the melting-pot has been employed instead of the cupola or reverberatory furnace; but Mr. Lassell seems to find the single melting sufficient: the only object of the second melting is to procure an alloy more dense and more free from pores; but that is a secondary object, compared to the all-important one, of eventually polishing the speculum successfully.

Believe me to be,
Truly yours,
General Sabine. Rosse.

XI. *Sir J. F. W. Herschel to General Sabine.*

Collingwood, Dec. 10, 1862.

My dear General Sabine,

I sent the photograph by last night's post, and I now return you Lord Rosse's, Dr. Robinson's, and Mr. Lassell's letters.

I concur entirely in the following points:—

1. To recommend the supplying of a 4-foot reflecting telescope to Melbourne, the large speculum to be of metal, not glass.

2. The mounting to be such as to be efficient for observing any object above the horizon with ease and safety.

3. The tube to be a skeleton one.

4. The cost, if possible, not to exceed £4500, but not less than £5000 to be applied for, as I really cannot imagine how it can be completed and conveyed to Melbourne and erected there for less than the former sum, leaving *any* profit to the artist employed.

5. That it is very desirable Prof. Wilson should come over to look to the construction, and to see the process of polishing and mode of observing.

6. That the means of repolishing the speculum should be furnished.

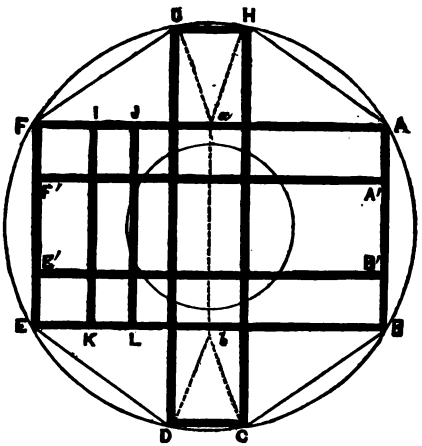
I have read the three letters with all due attention, but, after giving every weight to what Mr. Lassell says, I have still misgivings about the adaptability of the equatorial construction to the purpose contemplated, and I begin now to lean to the idea of an altazimuth mounting in wood with a good deal of iron bracing and tying about it—the round motion to be on a circular iron railway, and a system of pushing and pulling screw clamps to tighten up any local looseness of bearing, so as it were to *jam* the frame in the position it is to have during the observation, and to obviate all lateral *totter* arising from warping or shrinkage of wood, or expansion and contraction of iron. I cannot help fancying that an equatoreal so constructed as to pass across the pole would require such a length of support for the declination-axis as to be a fearful element of weakness, considering the enormous weight it would have to bear in all sorts of positions, and with *no possibility of counterpoising any part of it.* Nor can I quite reconcile myself to the mode of sustaining the observer when the telescope is directed to the zenith. Certainly Mr. Lassell's tower would have to be advanced bodily on its horizontal bar supports in a way and to an extent that I should not like to depend on.

Now in an altazimuth mounting on my father's principle, with addition of a framework for the counterpoises, there is no more difficulty or discomfort in observing in the zenith, or between the zenith and pole, or at or below the pole, than in any other direction. Any object in any part of the heavens can be equally well observed, and with a little contrivance, a slow motion to follow an object for a reasonable time without moving the framework in azimuth might readily be devised.

Such a mounting would be very far *cheaper* than an iron equatorial one (in which assuredly all the parts that are to sustain weights variable in direction ought to be forged in *wrought iron* with the utmost care). It has been proved efficient for the purposes for which such an instrument is wanted in the southern hemisphere—viewing and delineating nebulæ and comets, and discovering new nebulæ, &c. It will give places quite satisfying Dr. Robinson's postulate "within a small fraction of a minute" with dependable facility (and *that* even when so roughly constructed as my 20-foot stand—much more so when executed as works of the kind now are); and by differential observations questions of proper motion, &c. would be

fully within its reach. Any how, there must be no *daring experiment* incurring *risk of failure.*

From Lord Rosse's photograph it would appear that he has adopted Mr. Ramage's form of wood-work, in which the lateral stays rest on a short cross-beam, and are much more highly inclined to the horizon than the front and back ladders. This (as I have pointed out in my article on the Telescope in the Cyclop. Britannica) is an element of instability, and for a telescope of this size would, I think, be very objectionable. For the foundation frame I should be apt to propose something in the nature of the annexed woodcut, in which the thick



lines represent wood, and the thin iron,—the large frame ABEF A'B'E'F' being laid down first, and over it, connected by bolts, but not by any mortising or letting in to the same level, but *laid on it*, the cross pieces GD, HC of the transverse frame CDGH to support the lateral stays represented in projection by dotted lines, Ga, Ha, Db, Cb, attached aloft to *ab*, the ends of the cross top beam. These cross-pieces, with two others, IK, JL (equally laid on the lower beams and not under-cut), would form a bed for the rails carrying the lower end of the tube and the speculum—the whole revolving on a circular rail on rollers, and the centre work being connected with the wood frame by an interior iron *ring* bolted on to all the beams. The whole to be strengthened by diagonal ties of iron *ad libitum*.

I cannot conclude this without once more expressing my strong repugnance to the Cassegrainian and Gregorian construction, on the grounds expressed in my former letter. I feel certain that it would entail the sacrifice of much valuable observation from mere physical exhaustion in the observer.

I remain,
My dear General,
Yours very truly,
J. F. W. Herschel.

General Sabine.

XII. *The Rev. T. R. Robinson to Major-General Sabine.*

Armagh, Observatory, Dec. 15, 1862.

My dear Friend,

I send with this a letter from Mr. Grubb, in which he has stated some matters which he wishes me to make known to you. He has sent with it a photographic representation of his proposed construction, with references. His wish to make the aperture in the Cassegrain speculum smaller, arises from a desire to lessen the thickness of glass in the field lens of the lowest eyepiece (which he supposed to be 9 inches aperture), and for the same reason he wishes to make the aperture of this lens less in proportion to its focal length than I had assumed, namely 0·5. I find that, taking this proportion 0·4, and the focal length of the great speculum 32 feet, the aperture need only be 7·3 inches; but then the field of view will only be 11′ instead of 13′·9 with 8·11 inches, the aperture which I had proposed.

I thank you for the perusal of Sir J. Herschel's letter of the 10th inst. There is much of it in which I fully concur; but there are some parts which I think are open to objection.

1. I cannot at all doubt the practicability of equatorially mounting a 4-foot telescope: even if Mr. Lassell had not shown its practicability, it is a simple question of engineering; and I am sure any competent engineer, looking at Mr. Grubb's photograph, will see at once that *it* is quite adequate to carry steadily a tube which with its speculum need not be more than 4 tons weight. Mechanically it is far stronger than Mr. Lassell's; and, now that Bessemer's steel is in the market, by the use of that material in the declination-axis its strength may be doubled. I may also remark that, though the equatoreal does cross the pole, its declination-axis is of no dangerous length, and seems effectually counterpoised.

2. I am at a loss to conceive why Sir J. Herschel thinks a Cassegrain or Gregorian will cause such "physical exhaustion of the observer." The eye-end in Mr. Grubb's plan moves in a less sphere than that of a 12-inch achromatic; and I never heard that the Struves, Bond, or indeed any one who uses a refractor, find such a difficulty. On the other hand, I know by experience that climbing lofty ladders or even walking up and down stairs for a long night's work is really fatiguing.

3. If it were a question between an altazimuth mounting and *no southern telescope*, I would of course accept the former; but I think that to adopt it without absolute necessity is a real retrogression which would be bitterly repented, especially as we have no reason to suppose that the Melbourne authorities are not disposed to do their work well. *Let us not raise up difficulties.* It is undoubtedly true that in Sir J. Herschel's hands the 20-foot reflector has wrought wonders; but there are few indeed like him, and even he must excuse me for thinking that he could have done still more with it, had its mounting been equatorial. This at least follows from my own expe-

rience. On the one hand, I have worked a good deal with Lord Rosse's 3-foot, which is as steady and convenient as anything of the kind can be, and with Sir James South's 12-inch achromatic on a good mounting of the same kind; on the other, I am familiar with Mr. Cooper's great equatoreal; and I must say that there can be no doubt as to the superiority of the latter. Had it been altazimuth, Mr. C. could not have published his Ecliptic Catalogue for many years to come. I wish also to observe that I do not think a wooden mounting desirable for so large an instrument; even in these climates it is not durable, and under the hotter sun of Melbourne it would probably be less so; nor will wood and iron act well together under extreme changes of temperature.

It is obvious that micrometer measures off the meridian have much difficulty except with an equatoreal.

The Cassegrain has great advantage over the Newtonian in respect of celestial photography. The image in the focus of the great speculum must be magnified. Now if this be done by an eyepiece there are two evils. Glass stops a far greater proportion of the photographic rays than a speculum absorbs; and from the wide range of their refrangibility, they cannot be brought to an accurate focus. Quartz lenses would obviate the first, but not the second. On the other hand, the second speculum of the Cassegrain performs the magnifying without any help, and with perfect convergence of all rays to the same focus.

Sincerely yours,
T. R. Robinson.

Major-General Sabine.

XIII. *Mr. Grubb to Dr. Robinson.*

Dublin, Dec. 3, 1862.

My dear Dr. Robinson,

The plans and estimate which about nine years since I transmitted for the consideration of the Committee, at that time formed to report on the erecting of a powerful telescope in the Southern Hemisphere, having had the honour of being approved of by that Committee (although the design was never carried into effect on account of the British Government declining to supply the requisite funds), it seems desirable, now that the same Committee are reconsidering the matter at the request of some of the spirited inhabitants of Melbourne, that I should, as the successful projector on the former occasion, prepare for laying before the present Committee such data as circumstances occurring during the interval suggest.

I would then first state that nothing has presented itself to induce me to alter even in details the original design, much less to disturb my faith in its entire effectiveness. I have, however, made drawings to suit the latitude of Melbourne, a copy of which on a reduced scale, together with a descriptive reference, I propose shall accompany this

letter. Of the stability of the mounting I apprehend no doubt can arise, while its freedom of motion is ensured by the introduction of that peculiar system of equipoise first introduced in the large equatoreal exhibited by me in the recent International Exhibition. It will be seen that the inversion of the polar axis in the present design admits of using a comparatively small pivot for the lower bearing; while the near approach of the great mass of the entire moving weight to this pivot is most favourable to increased steadiness, and to both ease and accuracy of the movement in R. And if it be desired either now or at a future time to erect a telescope of 5-feet diameter (instead of 4-feet), I consider that the sizes given for the equatorial mounting would safely carry the larger telescope. It is, however, an error on the right side to have more than adequate strength and stiffness in the mounting, *more especially where such extra strength does not involve* any undesirable amount of labour, or any extra consumption of time in the required changing of the position of the instrument.

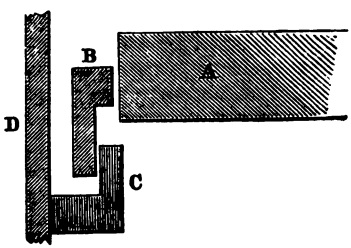
Tube.—A *ventilated* tube appears very desirable. The only drawing of one which I have seen (Mr. Lassell's) appears to be liable to considerable flexure; in short, if theory be good for aught, it must have a good deal—to which, and not to its stiffness, I would look to account for the apparent difficulty which I understand to exist of causing a displacement of the image by a pressure by hand at the upper end of the tube, the tube probably giving way at that end under the pressure, without causing the other end, including the large speculum, to follow. In the drawing which accompanies this letter I have indicated a construction for the tube, viz. "latticed," with internal diaphragms, which combines rigidity with lightness and some approach to elegance.

Details of focal length.—It may seem premature to speak here of these, but as some disadvantage has been suggested as pertaining to the use of a small metal of 9 inches diameter (in the 4-feet aperture of the large mirrors), I think it desirable to mention that in adopting the Cassegrain form of telescope, I consider that the focus of the large mirror need not be more than 8 diameters, or say 32 feet, and that in such case the lowest eyepiece may have a sufficiently large field (assuming the pencil it produces to be 0·2 in. diameter), the diameter of the small mirror, of the field lens, and of the hole in large speculum being made 7 inches only.

The supporting of the large speculum.—In respect of this important point, I fully expect it will be found that there is no necessity for incurring the inconveniences and disadvantages of an *elastic* support. It is I apprehend conceded that the system of equilibrated support, as originally devised by me, is adequate to the support at the *back* of the speculum. It appears also to be generally admitted that if the lateral support of the speculum be effected by its being *suitably* slung in an open hoop, or thus  , the same part being always below, no flexure is produced. If so, then any plan by which the "suitable"

slinging of the speculum in all positions would be ensured *ought* to be effective, *i. e.* should not induce flexure in the speculum. Now there may be several ways of effecting this; but I shall only describe that one which appears to be perfect in principle, and has been quite successful in practice in the case of a rather *weak* speculum, viz. of 20 inches aperture and 1·25 only in thickness.

This speculum, previously ground to a tolerably true figure on the back with the least possible reducing of its thickness, was next ground true on edge. The speculum being supported on the lever apparatus was ground and polished, and the lateral supporting effected as shown by the annexed diagram:—



in which A represents a portion of the speculum; B section of a trued ring or hoop having 2 diameters internally, the upper or lesser diameter being such as moderately embraced the speculum, a single thickness of fine cloth being placed between; C a second trued ring attached by screws to the inside of the speculum box, D, and finally trued after being permanently attached to D; B is fitted to C, not tightly, but so as to be free to move up and down, and B is prevented from bearing at bottom edge upon C by an elastic medium (or springs) between.

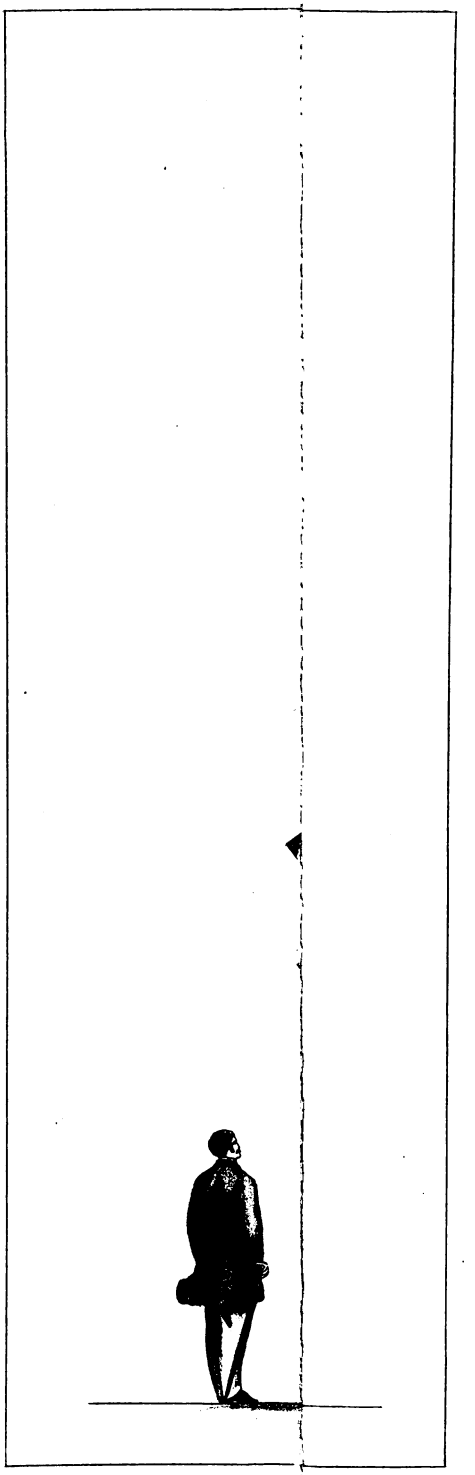
This method of lateral support appears to possess the following qualifications:—viz. 1st. It supports the speculum laterally, equally well in every position, and therefore no advantage results from keeping the speculum in the one position, nor *dis*advantage in rolling it into all positions. 2nd. Whatever portion of the speculum happens to be above is not in close contact with the surrounding hoop (B), and is therefore free to take its proper bearing on the back levers, while the opposite or lower portion (or edge) of the speculum, in pressing upon the ring B, discharges that portion of the ring B from contact with C. It may be said that, although free movement is thus provided for at top and bottom, it is not so at the sides; but it must not be forgotten that in regulating the tightness (or looseness) of the speculum in its ring B, and between the rings B and C, we have the power, to any desirable extent, of keeping the hoop B from pressing upon the sides of the speculum (near its horizontal diameter), and therefore there is no reason to conclude that this arrangement will not support a speculum laterally in *every position of its edge* with the same freedom from flexure *as the best regulated hoop will in a single position.*

Subsidiary Apparatus.—For the convenient removal and replacement of the large speculum, the apparatus intended to be furnished consists of two strong carriages, each moving on rollers (or wheels); one of these is provided with a fork raised or lowered by a strong screw, it is to be applied underneath the large counterpoise, the declination-axis being horizontal, and left there as a support in the absence of the speculum. The other carriage is provided with a suitable table, also raised and lowered by a screw, for supporting the speculum in its box while being either removed from, or attached to the tube of telescope; also of apparatus for enabling the observer to reach the eye-end conveniently in all required positions of the telescope.

I was surprised to learn that any difficulty had been felt with respect to such, and that it could be supposed that a complicated or expensive apparatus was required. The comparatively small distance of the place for the eye from the centre of motion, and the convenient height of this centre from the level platform surrounding the instrument are such, that for the great mass of observations the eye of the observer when applied to the eyepiece will range within from 3½ to 7 feet from the ground level. For this range I propose to furnish, first, a modification of the ordinary observing chair, and secondly, a carriage on which the aforesaid chair may be placed (run up upon), the carriage to be capable of being raised or lowered while the chair and observer are placed on it, and to allow of the observer reclining on the chair or standing, at pleasure, and with safety. For the few cases not met by the above, I think that a good step-ladder with a safety rail (made on the spot) will be adequate.

Hand motions.—The large diameter of the telescope would cause inconvenience to the observer in reaching to the edge while the eye is applied to the eyepiece, I therefore have planned arrangements for bringing the hand-wheels for such motions somewhere between the eyepiece of the telescope and that of the finder.

Estimate.—The residents of Melbourne having some time since desired to have an estimate including one large speculum (*only*), and *exclusive* of polishing machinery, I have estimated for *either such*, or with the additional large speculum (*ad libitum*). With respect to the prices named, I desire to make a few observations. I should think it imprudent on my part to name a sum which would only pay provided every thing went smoothly, and no such thing happened as a failure in the casting, or breakage in the completion, of the specula; and I should think it very indiscreet to name a sum which would curb me in executing the entire in a manner creditable and satisfactory to all concerned, including myself. The recent strides in the manufacture of steel would induce me to see how far this substance might be advantageously substituted (of course not without adding to the expense) in several parts where I had originally intended to use either cast or wrought iron—for example, the declination-axis, and the telescope-tube. Another observation I desire to make is, that without wishing in any way to detract from the merits and excellence of the workmanship of Mr. Lassell's large equatoreal, of which indeed I have only seen a lithographic drawing, and therefore could not



tend to form an opinion—I say without expressing any opinion to the workmanship, I think that any one at all conversant in such matters may see that the two constructions (viz. his and mine) are so entirely different *in character* that it would be quite impossible to judge of what one could be made for by knowing what the other cost. In his instrument the steadiness, whatever it may be, is attained by what I would call the brute force of tons of metal; in the other no expense is spared to keep down the weight and friction to the lowest. If my construction have (as I fully expect) decidedly greater steadiness, and it is moved with one-thirtieth of the force required to move the other, my labour in contrivance has not been in vain should it be put into use.

My original estimate was £4500. There are some things now added, and my present estimate will be £3800 for one speculum only and no polishing machinery, or £4600 for everything as before with the present additions. The difference between these two sums, or £800, would scarcely more than cover the actual cost of the items to be furnished for it; but it is not unlikely that in any case I should find it desirable to cast a second large speculum, and I place the advantage of taking the second speculum with the customer.

Faithfully yours,
THOMAS GRUBB.

Rev. Dr. Robinson.

References to the lithograph of Mr. Grubb's proposed Telescope.

A. Masonry (bed above ground).
B B. Masonry (piers).
C. Polar axis.
D. Sector (moved by the clock), 5 feet radius.
E. Clock (most part imbedded in pier).
F. Polar clamp. Clamping to polar axis and connected with sector D at F by a differential screw, for slow motion in A.
G. Hand-wheel for quick motion in A; it acts by a pair of bevil wheels on a shaft near I, having at top a spur pinion working in an internal wheel within the Hour-circle (H).
H. Hour-circle, 34 inches diameter.
I. Sector for relieving lateral pressure of lower bearing of polar axis; its action is adjusted by a screw acting with the intervention of a spiral spring upon a sliding piece, in which the pivots of the sector turn.
K. Roller for relieving lateral pressure of upper bearing of polar axis, its lever, weight, &c. being situate at further side of pier.
L L. Bell-crank, &c. for relieving *end* pressure of polar axis; a chain of peculiar construction is proposed for connecting polar axis and bell-crank.
M. Upper end of the lever belonging to the internal system of equipoising apparatus of declination-axis, &c.

N N N. Lever, weight, &c., supporting the upper end M of the aforesaid lever.
O. One of two levers relieving vertical (or rather polar) pressure of declination-axis.
P. One of the two roller-frames connected with levers O,—the other frame (that next the telescope) being hidden by the declination-clamp.
Q. A small part (the only visible) of declination-axis.
R. Declination-circle, 30 inches diameter.
S. Declination-clamp.
T. Telescope (tube 4½ feet diameter, 27 feet long).
U. Counterpoise.
V. Quick declination motion.
W. Slow declination motion.
X. Finder telescope, 4 inches aperture.

XIV. *The Earl of Rosse to General Sabine.*

The Castle, Parsonstown,
Dec. 18, 1862.

Dear Sabine,

I return the copy of Herschel's letter of the 10th, and have no hesitation in saying that I fully concur in his six propositions. In addition I should say that whatever form of mounting may be adopted, provided the instrument is accompanied by some one thoroughly competent to repolish the specula, the results will be important. Each form of mounting has its own peculiar advantages and disadvantages; and the choice may be made, without prejudice, to rest very much on financial considerations. Sir John Herschel leans to the altazimuth mounting; and no one knows better what can be done with it, even in its simple form without counterpoises, than he does. When thoroughly counterpoised, the whole stand moves in azimuth with the hand, and the telescope can be pointed without effort. A rack which gives a slow motion makes the micrometer sufficiently convenient for observations close to the meridian.

Sir John Herschel suggests more framework and more wheels than we employ; to that I see no objection: the additional cost would be nothing; and as to the additional weight, the counterpoise would sustain it at the expense of a little more central friction.

Sir John Herschel mentions Mr. Rammage's stand: I have seen it, and a very rickety affair it was. The wheels were much too close together, consequently it had but a very small base to stand upon. The timber was slight, badly put together, and there were no iron knees, or iron braces.

In the case of a counterpoised stand like that of my 3-foot telescope, the principal strain is from the *upward* thrust of the counterpoise lever, which acts upon a ring round the central point, and so raises the whole stand, leaving no more weight upon the wheels than is necessary to keep all steady.

The lateral stays, to which Sir John Herschel alludes, contribute to

resist that thrust, and are not necessary to resist lateral strain, which seems amply provided against by the distribution of the four wheels upon the circular rail, the wide stretch of the ladders at the base, the breadth of the back trussing, the large scantling of the timber, and the iron knees and straps. No gale has ever brought out any symptoms of weakness; and even when lifted bodily off the rail by the counterpoise-lever, there is no perceptible change of form. As to Mr. Lassell's equatorial mounting, I concur in Sir John Herschel's mechanical objections to it; still, considering Mr. Lassell's long experience, and the positive way in which he speaks as to its efficiency, I cannot doubt but that with its faults, whatever they may be, it would still do its work in a satisfactory manner.

It only remains to consider the German mounting: that, I confess, is my favourite; and were it my own case to be just commencing work in the Southern Hemisphere, I should adopt it, if financial considerations did not interfere. The fittings, however, should be for the Newtonian as well as the Cassegrain, lest the Cassegrain should not define perfectly. Sir John Herschel objects to the labour of observing with a Cassegrain, but Struve, Bond, in fact every one who employs a refractor has the same labour; and I am not sure whether moving up and down ladders with all the clothing necessary for a night's work is not more laborious. In choosing between the altazimuth and the equatorial, we must recollect that Sir John Herschel has had a life's experience with the altazimuth, and that no observer is likely to be able to use it as he did for a very long time.

With the equatorial the work is far easier; it will be done much quicker, and probably much better.

Believe me to be,
Faithfully yours,
Rosse.

General Sabine.

In consequence of Mr. De la Rue's absence from England, the President and Officers were obliged to prepare the draft of their report to the Duke of Newcastle without having the advantage of consulting him.

XV. *Extract from the Minutes of the Council of the Royal Society, December* 18, 1862.

The President and Officers submitted the following Draft of a Report to be transmitted to the Secretary of State for the Colonies, in compliance with His Grace's request conveyed in the Letter of Mr. Elliot, dated 10th October 1862.

"*Report of the President and Council of the Royal Society respecting the proposal of erecting in Melbourne a Telescope of greater optical power than any previously used in the Southern Hemisphere.*

"1. The President and Council learn with pleasure that the Board of Visitors of the Melbourne Observatory have proposed resolutions, indicating their sense of the importance of erecting at Melbourne an

equatorially mounted Telescope of great optical power, and that the proposal is favourably regarded by Sir Henry Barkly, Governor of Victoria, and by His Grace the Secretary for the Colonies. In respect to the importance which the President and Council attach to such an undertaking, they need do no more than refer to the fact that in the year 1850 the Royal Society and the British Association for the Advancement of Science presented a joint memorial to Her Majesty's Government, in which they urged the establishment of such a telescope at some suitable place in the Southern Hemisphere. The scientific objects to be attained thereby are so clearly stated in that Memorial, of which a copy is enclosed, and in the Resolutions of the Board of Visitors of the Melbourne Observatory, in July 1862, that the President and Council feel it unnecessary to do more than refer to these documents.

"2. Since the presentation of the Memorial of 1850 an equatorially mounted telescope of greater optical power than that then recommended has actually been constructed by Mr. Lassell, at his own expense, in England, and erected in Malta, where he is now occupied in making observations with it: we have now therefore in addition to our previous knowledge the benefit of his experience. In referring to Mr. Lassell's Telescope, the President and Council wish it, however, to be understood that they do not conceive that it should necessarily be copied in all respects; and that for the present they think it best to leave the details of construction in many respects open to further consideration.

"3. When the subject was previously under consideration, letters were written to some of the most eminent practical astronomers of Great Britain and Ireland, requesting them to state their opinions as to the best mode of construction; and a correspondence ensued, of which a printed copy is sent herewith. After receiving the communication from the Colonial Office of the 10th of last October, the President wrote to the four gentlemen who were appointed as a Committee on the former occasion to superintend the construction of the instrument (in case the Government should accede to the request), and also to Sir John Herschel, enclosing a copy of the former correspondence, and asking whether their views had in any way changed in the interval. The answers received from each have been circulated among the others, as was done on the former occasion, and have in most cases elicited additional remarks.

"4. Availing themselves of the information thus so kindly afforded them, the President and Council have to recommend as follows regarding the construction of the instrument contemplated.

"(*a*) That the telescope be a reflector, with an aperture of not less than four feet. This is essential, as no refractor would have the power required.

"(*b*) That the large mirror be of speculum-metal. Such mirrors can be constructed with certainty of success, and at a cost which can be foretold; whereas the recently introduced plan of glass silvered by a chemical process has not yet been sufficiently tried on so large a scale as that contemplated.

"(*c*) That the tube be constructed of open work, and of metal. Lord Rosse has recently changed the tube of his three-foot altazimuth from a close to an open or skeleton one, and it is understood that he intends doing the same with his great telescope. Mr. Lassell's tube is also an open one, which his experience leads him decidedly to prefer.

"(*d*) The telescope should be furnished with a clock-movement in right ascension.

"(*e*) Apparatus for repolishing the speculum should be provided.

"(*f*) With respect to the form of reflector to be adopted, some difference of opinion exists, as the Newtonian and Cassegrainian have each some advantages not possessed by the other. On this point further correspondence appears desirable; but as the main features of the scheme are the same in both cases, there does not appear to be any occasion to wait till this point shall have been finally decided.

"5. With respect to the cost, something must depend on the solidity of the construction and the perfection of the workmanship; but if it be assumed that the workmanship shall be of the best description, and the instrument furnished, as seems desirable, with polishing apparatus, and a second speculum for using while the other is being polished, it is probable that the cost will not fall much short of £5000.

"6. It is estimated that the construction of the instrument will occupy about eighteen months.

"7. It seems highly desirable that the future Observer should come to England during a part at least of the time occupied in the construction of the instrument, in order that he may become thoroughly acquainted with all its details, and especially with the mode of repolishing; and also that he may personally acquaint himself with the working arrangements followed at the Observatories of the Earl of Rosse and Mr. Lassell, who have expressed their willingness to afford him every facility."

Resolved,—That the Draft now read be adopted as the Report of the President and Council, and that the President be requested to transmit the same to the Duke of Newcastle.

Mr. De la Rue having returned to England was immediately made acquainted with the previous correspondence.

XVI. *Warren De la Rue, Esq., to General Sabine.*

The Observatory, Cranford, Middlesex, W.
February 15, 1863.

My dear General Sabine,

I have read the correspondence in respect of the proposed Southern telescope with considerable interest, and I return it to you under a separate cover.

I think that to adopt the altazimuth mounting in the present state of engineering science would be an inexcusable retrograde step. As regards the two forms of equatorial mounting submitted for consideration, namely, Mr. Lassell's and Mr. Grubb's, each presents certain peculiar advantages, and counterbalancing disadvantages. It is not a *necessary* consequence of Mr. Lassell's plan that the telescope cannot be placed within 30° of the pole, but must arise from faults of construction in the Malta equatoreal, for his two-foot telescope can be placed in a line with the polar axis. Moreover, a modification of the observing-tower would render it available for any possible position of the telescope. The strengthening cross-piece (the removeable piece) between the uprights on the polar axis would not have been necessary had the mounting been made sufficiently strong. Mr. Lassell does not go in for great niceties of fitting, hence the remarks in his communication about its forming no part of his plan to determine absolute places: I may here add that the necessary play in the supports of the great mirror allows of a minute difference of tilting, which is greater in effect than any errors dependent upon flexure, or from alterations in the position of the polar axis.

I think that it is absolutely essential that a system of supports similar in effect to that suggested by Mr. Lassell should be provided; the swinging ring does not prevent the distortion arising from the mirror bending by its own weight, when in any other position than pointed to the zenith. In order that the counterbalancing levers should act properly, the telescope must rotate in a cradle, so that the same radius may always be in a vertical plane. I have had no experience with the Cassegrainian form of telescope, but can imagine that this form would possibly afford means of correcting the aberrations of the large mirror by changes of curvature in the small mirror, but there would I conceive be a loss of light as compared with the Newtonian. On the other hand, the observer would be more conveniently placed for regulating the movements of the telescope and for directing it, although during the actual time of observing he would be placed at a disadvantage in comparison with the user of a Newtonian, which is undoubtedly the most pleasant to work with, from the perfect ease of the observer, who may sit down in a chair, looking straight before him, or at any convenient angle downwards, and is thus enabled to draw in the greatest comfort.

Dr. Robinson is in error in supposing that the Cassegrain telescope offers advantages for photography, the truth being that the second reflecting surface must be done away with altogether, and the sensitive plate placed directly in the principal focus of the great mirror. Lessening the time of exposure is of the greatest moment, and everything that conduces to that end is of the utmost importance. It is very easy to enlarge the image afterwards: latterly I have enlarged my lunar photographs from 1 inch (the size of the original negatives) to 38 inches.

A provision must be made for the removal from time to time of the second or small mirror, if it is contemplated to use the telescope for photography.

Moreover the driving clock will require especial attention: it must go smoothly, and must be capable of rapid adjustment to lunar, planetary, and sidereal time.

Mr. Grubb's photograph of a plan for mounting a 4-foot telescope evinces, as was to be expected from such a man, considerable originality and engineering ability, and I do not think that the Royal Society could do better than recommend that he be instructed to carry out the work. His estimate is moderate, and I should say that £5000 is a small sum for such an instrument as he would turn out of hand.

I may here mention that the only disadvantage of his form of mounting is the impossibility of moving the telescope all round the polar-axis, as the supports of the axis would stop it; this is very troublesome sometimes. In my own plan of mounting on the German system this is obviated.

As regards the durability of metallic mirrors, and the desirability of providing polishing machinery to be sent out to Melbourne, I have a word to say. My mirror A has been hard at work for nine years, and has been exposed to the destructive vapours of photographic chemicals for five years and a half, and is as good as ever it was. My diagonal mirror has been at work for fourteen years, and, although I have three others in reserve, I do not consider it necessary to replace it. I have taken the precaution to put boxes containing lime in my tube, and these, by preventing the deposition of moisture, have preserved my mirrors. My advice would be, let the observer use the telescope, and not trouble himself about acquiring the art of figuring mirrors—I defy him to acquire the art in less than from four to five years. The possession of a polishing machine will be at first of about the same advantage as the possession of a complicated turning lathe would be to a gentleman desiring to become an amateur turner. Each must buy his experience by trial and failure. The finest figure may be spoiled in half an hour, and without the necessary knowledge will not be readily regained. Even for the most experienced the difficulties of producing critically fine figures are very great.

I have had a little experience with a silvered glass mirror of small size by Steinheil. It was placed in my observatory, and care taken of it, but in less than two years the silvering underwent a molecular change, and broke up into minute scales. I think that glass mirrors should only be used by persons capable of resilvering them; at all events until their very general employment brings about the establishment of commercial facilities for this being readily accomplished.

I have written these remarks hurriedly, but I shall be much pleased to afford any other information which you may think I can furnish from my general experience in making and using reflecting telescopes.

I am, yours very sincerely,

General Sabine. WARREN DE LA RUE.

XVII. *Sir J. F. W. Herschel to General Sabine.*

Collingwood, February 22, 1863.

My dear General Sabine,

I return you Mr. De la Rue's letter, which I have read with much attention.

If the equatorial form of mounting can really be rendered efficient so as to carry the telescope up to and beyond the pole and reach every point of the visible hemisphere with safety and comfort, as Mr. De la Rue thinks, there is no more to be said. I have still some misgivings, but I have no experience of such heavy mountings, and in such matters experience is everything.

Mr. De la Rue's remark as to the play in the support of the great mirror, is of very great importance. I see no possibility of *avoiding* such play. The remedy is clear, and, as I conceive, imperatively necessary to be applied, viz. the use of a fiducial collimator, as used by myself, and described in Phil. Trans. 1833, p. 488, and in my 'Cape Observations,' Introduction, p. xiv. I wonder I did not think of insisting on this point in my previous letters on this subject. Of course the mirror must be capable of adjustment to the collimator by screws moveable at any moment by the observer *at* the eyepiece.

I do not quite understand Mr. De la Rue's remark on the system of supports for the mirror. It seems to me that without a swinging ring *no* system of back support can be effectual. With it a mechanism would certainly, as he states, be required to keep the point of its suspension in a vertical plane.

It is very comfortable and reassuring to have both Mr. Lassell's and Mr. De la Rue's testimony as to the permanence of polish of their specula. This being the case, the observations of the latter on the non-necessity (and indeed positive disadvantage) of supplying polishing machinery appear to me worthy of all consideration.

I quite agree with Mr. De la Rue in deprecating the idea of adopting silvered glass for the great mirror. It would be a most hazardous experiment.

I remain,
My dear General,
Yours very truly,
J. F. W. Herschel.

General Sabine.

XVIII. *The Earl of Rosse to General Sabine.*

The Castle, Parsonstown,
March 13, 1863.

Dear Sabine,

I enclose De la Rue's and Herschel's letters, which I have read with much interest. As to De la Rue's, I perfectly coincide with him in everything contained in the early part of his letter. Our 6-foot mirror certainly tilts a little, which would vitiate all *absolute* determination of polar distances without the aid of Herschel's colli-

mating arrangement. The tilting affects also Right Ascensions, but in a very slight degree, for which the collimator is also the remedy, differential observations of course affording the ultimate check.

We have never made any effort to annihilate tilting; Mr. Grubb's arrangement of the supporting apparatus seems much better than ours with the view to that object. Some inconvenience may perhaps be felt from *sudden* changes in the position of Mr. Lassell's polar axis. We have never succeeded in detecting strain referable to edge-pressure. As to Mr. De la Rue's remarks on the durability of mirrors, no doubt he is correct if the mirrors *are always dry*; so long as they can be kept dry they are practically everlasting. My experience, however, is, that in the hands of assistants they are frequently wet, and I am not sure whether with the utmost care I could take, unless I were to resort to some artificial means of keeping their temperature above that of the air, I could secure them from the deposition of dew arising from sudden changes in the condition of the atmosphere while they were unsecured.

To employ quicklime in a lattice tube would be out of the question, and in a close tube I have never thought the action would be rapid enough to be effectual. From the ascending and descending currents in a close tube I have always thought that the whole air would be so often changed that quicklime would be of no use. When the speculum is covered quicklime is effectual; and telescopes under a dome are seldom dewed. My remarks apply to this climate; how it might be in Australia I cannot tell, but so long as the speculum preserves a fair polish it would be better not to meddle with it; and while I recommend a polishing-machine to be sent out, I should be very sorry to encourage its being employed unless when absolutely necessary. I should advise also that a small polishing-machine should be sent with two 10-inch mirrors for the astronomer to practise upon. This would be a trifle; though without some instruction in addition to that to be found in books, it might be very uphill work to acquire the necessary tact to produce a fine speculum; still I think machinery affords such facilities that with a little instruction any educated man, with an aptitude for mechanical pursuits, would soon acquire the knack so far as to succeed moderately.

Believe me to be,
Truly yours,
ROSSE.

General Sabine.

XIX. *Warren De la Rue, Esq., to General Sabine.*

The Observatory, Cranford, Middlesex,
April 7, 1863.

MY DEAR GENERAL SABINE,

I return you Lord Rosse's letter, which I have read with great interest. I agree with Lord Rosse that the collimation-apparatus of Herschel would afford the means of obtaining reliable determi-

be revealed. The molecules of the sensitive material have a certain size, below which photography cannot go. Subsequent enlargement of such a picture cannot develope what does not exist in it. Besides, a copy which is enlarged by lenses cannot be so sharp as one by metallic reflexion. No combination can be so achromatized that a large portion of the most refrangible rays shall not be aberrant; and though the most photographic of these do not go far beyond the line G, yet Dr. A. Miller's spectra show that others to a great distance beyond H have enough of energy to blur an outline. Now as the proposed Cassegrain would give an image equivalent to a focus of 156 feet (for the moon 17½ inches diameter), and as the light of a 4-foot mirror will probably suffice for this, I hope it will be admitted that my *error* is not without excuse.

Fourthly, I still hold, after considering all that has been said, that a polishing-apparatus is necessary. The first of the two specula would of course be used until it becomes so tarnished as to impair seriously its efficiency. Then it should be changed for the second mirror, and *then* experiments should be made on repolishing it. As by a recent contrivance of Lord Rosse even a 6-foot can be tested while on the polishing-machine, these experiments would not interfere with the continuous use of the telescope; and I am confident that long before the second one became unserviceable the first would be again fit for work. But I certainly wish that there should be attached to the instrument an assistant who, besides being able to observe, should be already trained to the work of polishing.

I hope we shall soon hear from Melbourne, and hear good tidings; for, as Sir John Herschel has well remarked, the days that pass before this survey of the Southern Heaven begins are each an irretrievable loss to Astronomy.

Sincerely yours,
T. R. Robinson.

Major-General Sabine,
President of the Royal Society.

XXI. *Sir J. F. W. Herschel to General Sabine.*

Collingwood, April 24, 1863.

My dear General Sabine,

Lord Rosse's letter of March 13, and Mr. De la Rue's of April 7, are herewith returned.

I am glad to see that it appears agreed, 1st, that the collimator shall be adopted; 2nd, that a lattice tube shall be employed.

I confess I had been astonished to hear that the metal now in use absolutely resisted tarnish. It now appears, however, that when *dewed* it suffers. And that it will be dewed whenever during its exposure a change of temperature from cold to warm, accompanied with a degree of moisture approaching the dew-point, happens is certain, whether the tube be latticed or not. Therefore I see no help but to send out a polishing apparatus. But then this entails as a necessity that the observer must be *a man of skill and*

energy, giving his whole heart, soul, time, and thought to the one great work of using and keeping in order this magnificent instrument. 2ndly. That he must learn to polish, and must on no account meddle with the *great* speculum till he has *perfectly* familiarized himself with the operation of polishing and figuring specula by practice on smaller ones; and for this I can hardly believe that a 10-inch size will be large enough. 3rdly. That the great mirror should not be opened, even for inspection (further than to be satisfied that it arrives *unbroken*), till the whole instrument is erected and quite ready for use, and that the greatest diligence should be then employed to secure a great harvest of observations while the surface is yet perfect, as it is more than probable that the first attempt (and many more) to renew it will prove failures. And this is all that occurs to me on the subject of these two letters.

I remain,

Yours very truly,

J. F. W. HERSCHEL.

XXII. *Mr. Lassell to General Sabine.*

London, July 22, 1863.

DEAR GENERAL SABINE,

On the occasion of my temporary visit to England I have had the opportunity of looking into some of the correspondence respecting the proposed 4-foot telescope for Melbourne, and, in consequence, I should be glad to be allowed to state that I do not intend to continue my observations with the telescope of this size now erected in Malta, and described in this correspondence, beyond the period of twelve, or at most eighteen months from the present time, and that, if this equatoreal should meet the requirements of the Melbourne Committee, I shall then be glad to place it at their disposal.

I remain,

My dear Sir,

Very truly yours,

General Sabine, R.A. WILLIAM LASSELL.

XXIII. *Warren De la Rue, Esq., to General Sabine.*

(Extract.)

MY DEAR GENERAL SABINE, August 8, 1863.

While admitting the correctness of Dr. Robinson's opinion, "that there must be a limit as to the least magnitude which can be impressed on even the most sensitive collodion," I am able to say that the minuteness of details in astronomical photographs made with a 13-inch reflector is not limited by this cause. In a 4-foot reflector the image would be nearly four times as large, and the structure of the collodion would consequently interfere in a proportionately less

degree with the perfection of the image; I therefore urge on the attention of the Committee the advisability of making arrangements for the removal of the second mirror, and of placing the photographic apparatus in the focus of the great mirror.

My driving-clock will keep the image of a star on the wire of a micrometer for several minutes, so that the difficulty in photographing celestial objects does not result, in my case, from imperfections in the driving-clock; it arises solely from atmospheric disturbances. I am persuaded that the proper direction to be pursued in developing astronomical photography will be to increase the aperture of the instrument in relation to its focal length, and to diminish the term of exposure as much as possible by also improving the chemical part of the process.

I am, yours very sincerely,

General Sabine. WARREN DE LA RUE

TAYLOR AND FRANCIS, RED LION COURT, FLEET STREET.

[*For private circulation only.*]

THE ROYAL SOCIETY.

CORRESPONDENCE

CONCERNING THE

GREAT MELBOURNE TELESCOPE.

PART III.

CORRESPONDENCE.

I. *General Sabine, P.R.S., to Professor Wilson.*

The Royal Society, Burlington House,
September 23, 1863.

MY DEAR SIR,

The Council of the Royal Society have ordered the printing of the Correspondence of some of their Members regarding the Melbourne Telescope, upon which their Report to the Duke of Newcastle was based. It forms a small octavo pamphlet under forty pages, with lithographs of Mr. Lassell's Telescope and its mounting, and of Mr. Grubb's proposed Telescope and its mounting. We shall receive the copies from the press in ten days or a fortnight, when I shall hope to send you thirty or forty through the Colonial Office for distribution amongst those who you may judge are likely to be influential in promoting so important a matter. As I presume Sir H. Barkly will have left you for his new Government, I shall send a copy addressed to him at Mauritius. If either the Astronomer (Mr. Ellery) or you wish for more copies you have only to write for them. I do not apprehend that there would be any difficulty in recommending a properly trained, and in other respects suitable, assistant from this country competent to repolish the speculum in case of need, and to both drawing the nebulæ and photographing (his remuneration must of course be from the Colonial funds). I have heard two persons spoken of, either of whom I should judge would be very eligible,—one is Mr. Alexander Herschel, a son of Sir John, the other is a son of Mr. Grubb, who would be thoroughly at home in all particulars. The estimate which is entertained here of the enormous and increasing wealth of your colony would stand very much in the way of any proposition for supplementing the Colonial appropriation.

(Signed) EDWARD SABINE.

II. *General Sabine, P.R.S., to His Grace the Duke of Newcastle.*

The Royal Society, Burlington House,
October 1, 1863.

My Lord Duke,

Adverting to the despatch from the Governor of Victoria respecting the erection at Melbourne of a Telescope of greater power than any previously used in the Southern Hemisphere, and to the Report thereon furnished at your Grace's desire by the President and Council of the Royal Society, I have now the honour to request your acceptance of a copy of the Communications received from the Fellows of the Society who were consulted by the President and Council on that occasion, and which have been printed by direction of the Council. Should your Grace be of opinion that copies of these letters might be advantageously presented to those persons at Melbourne with whom the proposition of establishing a telescope of this description originated, the Council would be happy to supply Mr. Elliot with as many copies as he should think it desirable to send out.

(Signed) Edward Sabine,
President R. S.

III. *General Sabine, P.R.S., to Professor Wilson.*

The Royal Society, Burlington House,
August 24, 1864.

My dear Sir,

I did not receive your letter written officially, as Chairman of the Board of Visitors of the Melbourne Observatory, until the day after the departure of the Southampton Mail. I hope that I am in time for the Marseilles Mail. You are already in possession of the opinion of those Fellows of the Royal Society who are generally esteemed as most competent to judge on the comparative suitability of Mr. Lassell's or Mr. Grubb's Telescopes for the purposes which are contemplated at Melbourne. The opinions are expressed fully and unreservedly, and several copies of the Correspondence have been sent officially to Melbourne through the Secretary of State. The decision in regard to the Telescope to be preferred rests, and must rest, with the Melbourne authorities. All that I am able to say is that I have no reason to suppose that any change whatsoever has taken place subsequently in the opinions expressed in the Correspondence which is in your hands.

(Signed) Edward Sabine.

At the Meeting of Council, October 27, 1864, the following correspondence was presented:—

IV. *Prof. Elliot to the Secretary of the Royal Society.*

Downing Street, 31st August, 1864.

SIR,—With reference to previous correspondence respecting the large Telescope proposed to be erected at Melbourne, I am directed by Mr. Secretary Cardwell to transmit to you, to be laid before the President and Council of the Royal Society, a copy of a Despatch from the Governor of Victoria, forwarding a Letter from the Secretary to the Board of Visitors of the Observatory, requesting that the thanks of the Colony should be presented to Mr. Lassell for the offer, which it appears has been made through General Sabine, to bestow upon the Colonial Government the powerful telescope which that gentleman possesses, provided that it be found suitable for the work for which it is required.

It will be observed that the Board of Visitors are of opinion that the suitableness of the instrument can be best determined by the Council of the Royal Society; and I am to add, that Mr. Cardwell would be glad to learn that it was in the power of the Council of the Royal Society to accede to the wish of the Board of Visitors.

I am, &c.,

(Signed) FRED. ELLIOT.

The Secretary of the Royal Society.

(Copy.)

V. *Governor Sir C. Darling to Mr. Cardwell.*

Government Office, Melbourne,
22nd June, 1864.

SIR,—I have the honour to transmit herewith the copy of a Letter and Enclosure from the Secretary to the Board of Visitors of the Observatory, requesting that the thanks of the Colony should be presented to Mr. Lassell for the offer, which it appears has been made through General Sabine, to bestow upon the Colonial Government the powerful telescope which that gentleman possesses, provided that it be found suitable for the work for which it is required.

2. The Board of Visitors are of opinion that the suitableness of the instrument can be best determined by the Council of the Royal Society of London, who have already been good enough to interest themselves in the matter, and have expressed their hope that effect may be given to their views on these two points through the medium of the Secretary of State for the Colonies.

3. My advisers concur in the desire entertained by the Board; I have, therefore, respectfully to express my hope that you will cause the necessary communications to be made to the Council of the Royal Society, and to Mr. Lassell, who is, I believe, a Member of that Association.

I have, &c.,

(Signed) C. H. DARLING.

The Rt. Hon. Edw. Cardwell, M.P., &c.

(Copy.)

VI. *Prof. Wilson to the Chief Secretary.*

The University, June 22nd, 1864.

Sir,—I have the honour, by direction of the Board of Visitors, to forward the enclosed Extract from the Minutes of a Meeting held yesterday on the subject of the Equatorial Telescope offered as a present to the Colony by Mr. Lassell, and to request that you will have the goodness to comply with the recommendation of the Board.

The first direct intimation of Mr. Lassell's liberality was in a letter from Major-General Sabine, President of the Royal Society of London, received by the last mail, in which it is also stated that Mr. Lassell presents his Telescope only conditionally on its being found suitable for the work for which it is required, and on its being actively used.

As regards the first of these conditions, the Board does not possess sufficient information to justify it in expressing an opinion, and considers that it will be most in accordance with what is due to Mr. Lassell and best for the interest of science that the same Committee of the Royal Society of London which has already taken so active a part in the matter, should be requested to consider and decide upon the point.

Whatever that decision may be, however, the Board feels that no time should be lost in expressing to Mr. Lassell the thanks of the Colony for his very munificent offer.

I have therefore the honour to convey to you the wish of the Board of Visitors that you will have the goodness to request his Excellency the Governor to carry out these two recommendations through the Secretary of State for the Colonies.

I have, &c.,

(Signed) W. P. Wilson,

Hon. Sec. to Board of Visitors.

The Honourable the Chief Secretary.

(Copy.)

Extract from the Minutes of a Meeting of the Board of Visitors of the Observatory held June 21st, 1864.

Resolved,—1st. That the thanks of the Board be given to Mr. Lassell for the offer which he has made of his Telescope for the Melbourne Observatory.

2nd. That the substance of this Resolution be expressed in a Letter to the Honourable the Chief Secretary, accompanied by a request that he will forward it through his Excellency the Governor to Mr. Lassell.

3rd. That the Council of the Royal Society of London be requested to decide upon the fitness of Mr. Lassell's Telescope for the work of this Observatory.

(Signed) W. P. Wilson,

Hon. Sec.

(Copy.)

VII. *General Sabine to Mr. Elliot.*

Llandovery, Sept. 26, 1864.

SIR,—Your letter of the 31st of August addressed to the Secretary of the Royal Society on the subject of the proposed Melbourne Telescope has been forwarded to me in the country, the Royal Society being at present in recess. I notice this in explanation of the delay which has taken place in the present reply; under these circumstances I have not thought it expedient to incur the loss of time which would be involved in waiting for the re-assembling of the Council in order to send a more formal reply.

The previous correspondence on this subject printed in 1863 contained the opinions of several persons, of the greatest experience in this country in the construction and use of large reflectors, on the most suitable construction, both as to the optical part and to the mounting of such a telescope for the purposes contemplated at Melbourne, together with its probable cost and the time required for its completion. Full descriptions were given of Mr. Lassell's reflector, and of one differing from Mr. Lassell's in several particulars, which had been proposed by Mr. Grubb on a former occasion when the employment of a reflector of great optical power in the southern hemisphere was under consideration. The relative merits of Mr. Lassell's and Mr. Grubb's telescopes for the requirements of the Melbourne Observatory were most fully and most unreservedly discussed in the correspondence printed in 1863. A copy of that correspondence was presented to Mr. Cardwell's predecessor, the Duke of Newcastle; and subsequently, with his Grace's approval, several copies were sent to the Colonial Office for distribution in the Colony itself. I have now the honour of enclosing an additional copy for Mr. Cardwell's perusal.

Viewed simply as a *scientific* question, few persons after the perusal of the correspondence would, I apprehend, dissent from the conclusion that Mr. Grubb's telescope was entitled to a preference for the special locality and purposes contemplated at Melbourne, leaving that of Mr. Lassell for continued use in the hemisphere in which it was constructed, either by Mr. Lassell himself, or by others to whom it should be his pleasure to entrust it. But Mr. Lassell's munificent offer (of which too much cannot be said in praise), of freely presenting his telescope to the Melbourne Observatory if it should be deemed by the Visitors of that Observatory as fully meeting their requirements, has introduced a new element, viz. a *money* element into the consideration, and as the question now stands, I apprehend that, whatever might be the opinion of the Royal Society or of the Board of Visitors at Melbourne, the *decision* must remain, and should properly rest, with those who will have to supply the money, viz. with the authorities and the Legislature of the Colony itself.

It should always be borne in mind that with a due regard to the importance of the researches in which the telescope is to be employed

at Melbourne, and to the number of years in which it will probably continue in work, the first cost of the instrument itself is relatively of less importance.

I have, &c.,

(Signed) EDWARD SABINE, P.R.S.

VIII. *Governor Sir C. H. Darling to Mr. Secretary Cardwell.*

(Copy.)

Government House, Melbourne,
August 7, 1865.

SIR,

With reference to your despatch of the 6th October 1864, No 61, I have the honour to enclose the copy of a letter to my private Secretary from the Secretary of the Board of Visitors of the Melbourne Observatory, by which you will perceive that the Legislature have determined upon purchasing a new Equatorial Telescope, and that it follows that this Government will decline to avail itself of the generous offer of Mr. Lassell to present his Telescepe to the Colony.

2. As you have been good enough to direct that the previous communications on the subject should be made to Mr. Lassell by your authority, I am perhaps justified in expressing a hope that you will cause an intimation of the final decision in the matter to be communicated to that gentleman with a renewed expression of the thanks of the Colonial Government.

3. I may take the opportunity of stating that the sum of £5000 has been voted for the purchase of a new telescope.

I have &c.,

(Signed) C. H. DARLING.

The Right Honourable
Edward Cardwell, M.P.

IX. *Prof. Wilson to General Sabine.*

The University, Melbourne, Aug. 21, 1865.

MY DEAR SIR,—It is with very great satisfaction that I forward to you the following resolutions of the Board of Visitors, adopted on the 15th inst. :—

I. That the President of the Royal Society of London be informed that the Legislature of Victoria has voted the sum of £5000 for the purchase of an Equatorial Telescope, one-half of which has been already remitted to the Crown Agents in England, and placed at the disposal of Major Pasley, R.E., for the purpose, and that the Government has placed the correspondence connected with it in the hands of the Board of Visitors.

II. That the President and Council of the Royal Society be requested to give the Board of Visitors the benefit of their assistance in selecting a maker, settling the contract, and superintending the

construction of the telescope, so as best to carry out the recommendations contained in the Report of the Royal Society to the Duke of Newcastle, 18th December 1862.

III. That Major Pasley be requested to place himself in communication with the President and Council of the Royal Society, and, after ascertaining their views, to enter into such contract as will most effectually carry them out.

I enclose also a copy of a letter received from the Treasury, on which the foregoing resolutions are based, and a copy of the letter which I send to Major Pasley by this mail.

The great interest which you have shown in this matter leads the Board of Visitors to count confidently on your further assistance in bringing it to a successful conclusion. The request contained in the second resolution is not intended to imply that in the opinion of the Board any further discussion as to the form of telescope or the maker is necessary. The Board thinks, and I believe that it is also your opinion, that the discussion which has already taken place has settled that question, and that Mr. Grubb's proposal should be adopted. This is not distinctly expressed in the resolution, because Mr. Grubb's name is not mentioned in the Report of the Royal Society, and because the Board desires to leave you free in the event of anything having happened to Mr. Grubb, or of any discovery having been made which would tend to modify your opinion.

In any case, the Board, bearing in mind the great length of time that has elapsed since the proposal for a telescope was first made, and having now received authority from the Government to act in the matter, is desirous of securing the completion of the telescope at the earliest possible time consistent with the highest attainable perfection in the instrument, and considers that this end will be most effectually secured by leaving you quite free to act in the matter, and trusting to you to secure the cooperation of those eminent practical astronomers whose names you mentioned as willing to superintend the work during its execution.

Mr. Grubb's last estimate is £4600 for the telescope complete; and this I believe covers everything, including the erection in Ireland for a trial.

The sum voted is £5000, and the balance, £400, will be available for a spectroscope and for a photographic apparatus adapted to the telescope, and will still probably leave sufficient to pay the freight to Melbourne. As these two adjuncts will not occupy long in making, it will probably be desirable not to commence them till the telescope proper is approaching completion, so that the latest improvements may be introduced into them.

Trusting to your earnestness to induce you to undertake the great amount of trouble we are imposing upon you,

I remain, my dear Sir,

Very faithfully yours,

W. P. Wilson,

Hon. Sec. to the Board of Visitors.

Major-General Sabine, R.A., Pres. R.S.

X. *Professor Wilson to Major Pasley.*

The University, Melbourne,
Aug. 21, 1865.

My dear Major Pasley,

I have received from the Treasury a letter (No. 4268, Aug. 11, 1865) stating that the Crown Agents have been informed that £2487 13*s.* 0*d.* in their hands is to be applied towards the purchase of an Equatorial Telescope, and that the balance, £2512 7*s.* 0*d.*, will be forwarded when required. This letter, a copy of which is enclosed, also states that it is to be expended under your order, and that you have been informed that you will receive further communications on the subject from the Board of Visitors to the Observatory.

With reference to this matter, I have the honour, by direction of the Board of Visitors, to forward to you the following resolutions of the Board adopted on the 15th inst.

[The resolutions are the same as in the preceding letter.]

To render the present position of the matter clear, it is necessary to enter into a brief history of what has taken place. You will probably remember that for many years I have been endeavouring to induce the Government of Victoria to procure a large reflecting telescope for observing the nebulæ of the Southern Hemisphere. On the 23rd July, 1862, at the request of the Board of Visitors, Sir Henry Barkly wrote to the Duke of Newcastle to ask through him a Report on the subject from the Royal Society of London. This led to a correspondence amongst those Fellows of the Royal Society best acquainted with the subject, in which the form of telescope most suitable for the purpose was fully discussed in all its details, and the Report referred to in the second resolution was adopted on the 18th of December, and forwarded through the Duke of Newcastle to Melbourne. Subsequently Mr. W. Lassell offered his large telescope as a present to the Melbourne Observatory if it should be found suitable in all points, and considerable time was occupied in correspondence on the subject. At last, however, the matter has been decided; on the recommendation of the Board of Visitors the Government placed £5000 on the estimates for a new telescope, and the Assembly voted it; and half of this sum, which had been sent to England for the alteration of Mr. Lassell's telescope, is now available.

The Government has now authorized the Board of Visitors to communicate directly with the Royal Society and the maker of the telescope in order to avoid the delay of sending the communication through the Governor and the Secretary of State as formerly; and the Board relies on your good offices, as one of its Members, to expedite all matters of business connected with the procuring of the telescope.

The form of the telescope and all details of construction have been so fully discussed by the Royal Society, and the superiority of the mounting proposed by Mr. Grubb, of Dublin, has been so clearly established, that little remains to be done besides giving him the

order. I write by this mail to General Sabine on the subject, and from the interest which he has all along shown in it, I am sure he will do all in his power to relieve you of unnecessary trouble. If, as I expect, he agrees with me that the Royal Society has practically decided that Grubb's form of telescope is that which should be adopted, the best plan will be to set Mr. Grubb at work at once, so that no further time may be lost,

I remain,
My dear Major Pasley,
Yours very faithfully,
(Signed) W. P. Wilson,
Hon. Sec. to the Board of Visitors.

XI. *Rev. Dr. Robinson to General Sabine.*

North Great Georges Street, Dublin,
Feb. 17, 1868.

My dear Friend,

We had our first Committee Meeting to-day, and I am glad to tell you all went on smoothly; De La Rue is delighted with the details of the telescope, and much pleased with the photographs, of which Howard Grubb took a few in the rare "lucid intervals" that have occurred, one of which I enclose. He appears also, from two stars which we saw to-day, to think the figure and polish of the speculum very good. To-night promises to be tolerably favourable, and if so, I see no reason why we should not agree on the heads of our Report to you to-morrow; in which case I will lose no time in drawing it up and in forwarding it to you. I read Mr. Verdon's letter to you; it authorizes us to hope that the roof will be made, but I must own I have my doubts about the economy of constructing it at Melbourne. In particular the very effective arrangement of rollers on which it is proposed it should move will require first-rate workmanship.

Yours ever,
T. R. Robinson.

General Sabine.

At the Meeting of Council of the Royal Society, March 19, 1868, the Report of the Committee on the Melbourne Telescope to the President and Council, with a separate Report by Mr. Lassell, was presented, as follows:—

The Committee were informed by Mr. Grubb at the close of last year that the Telescope was ready for their final examination; but the bad weather which has prevailed in Ireland ever since precluded all trials of its optical power till February 17, when they met at Mr. Grubb's works in Rathmines Road.

1. The Telescope was not finished at the time named in the contract; but the Committee have ascertained that the delay arose

solely from unfavourable weather, which not only impeded the actual work of polishing, but for weeks together made it impossible to test the figure of the specula. They considered that it was far more important to send out a perfect instrument than to keep the exact time.

2. The Committee, after minutely and carefully studying the mechanical details of the equatorial, have come unanimously to the conclusion that it is a masterpiece of engineering. Its movements are surprisingly smooth and steady; it can be moved to any portion of the sky, even if it have to be reversed from one side of its pier to the other, in less than a minute by two operators and with very little exertion.

The clock is smooth and equable in its action, it is very powerful, and quite equal to its work. Great change of rate, as from sidereal to lunar time, is effected by an ingenious piece of differential gearing; small changes are made by a cam adjustment; moreover it rings seconds, for the double object of comparing its rate with a chronometer and to assist the observer in his observations.

3. The Committee are strongly impressed by the great convenience to the observer of the arrangements of the hour and polar-distance circles, the facility of controlling their adjustment, and the easy access to the eyepiece.

4. The stability of the tube was severely tested, both in respect of its general stiffness and its power of resisting torsion, such as might be produced by the weight of the small speculum when the Telescope is off the meridian; and the results were highly satisfactory.

5. In large reflecting telescopes it is usual to make provision for keeping a given diameter of the great speculum always in a vertical plane. When they are equatorially mounted, this is done by rotating the tube in its cradle. Here the tube does not turn; but there is a special arrangement of hoop-suspension, by which, whatever diameter may be vertical, it is supported in a uniform and symmetrical manner. The system of triangular levers at the back of the great speculum is also contrived so as to prevent them from exerting any pressure which might distort it. This is a matter of the highest importance, and the attention of the Committee was specially directed to it. They examined it most carefully, putting the Telescope in various positions of $\mathcal{R}$ and P. D. on each side of the meridian, both by day and night, and could not find *any sign of flexure or any distortion of the image in any of these changes.*

6. In order to test the optical power of the second speculum B (the first one, A, had been tried and approved by a member of the Committee on October 12 last), the Telescope was directed to the following objects:—In the daytime Venus and α Andromedæ; at night (which fortunately was clear and steady) Castor, the Great Nebula of Orion, ζ Orionis, γ Andromedæ, Uranus, 1 Messier, 37 Messier, 46 Messier, and 51 Messier, from which it will be seen that both the light-collecting and defining powers of the instrument were fairly tried. The powers used were 220 (the lowest which

can take in the entire pencil), 350, and 450, all negatives. Of course one would not propose such an instrument for the measurement of close double stars, work for which Telescopes such as those of Poulkova and Harvard are possibly better fitted; but the Committee found that the light even of large stars was collected into small, hard, and perfectly circular disks, free from rays; and though some diffused light* surrounded them, it was exactly concentric with the central disks. The 5th and 6th stars of the Trapezium of Orion were not only plainly seen, but were very bright; ζ Orionis was well shown, and the companion of γ Andromedæ was clearly divided with the powers of 350 and 450, and the different tints of the components were evident. Uranus was well seen, but was surrounded by such a multitude of very minute stars that, without access to the tables of his satellites, it was impossible to know whether any of them were seen†. 37 M. was broken into a heap of stars so large and brilliant that it quite lost the character of a cluster. The planetary Nebula in 46 M. brought out most strikingly the light-collecting power of this Telescope; for it (which in most Telescopes appears as a faint disk) was revealed as a ring, bright even on the dazzling ground of the surrounding stars, which here were as brilliant as the components of the Pleiades appear in ordinary instruments. With respect to the Nebulæ, it is needless to say more than that Lord Rosse considers its performance in bringing out the details of the Orion Nebula, 1 M. (the Crab), and 51 M. (the Great Spiral), quite satisfactorily.

7. The Committee had no opportunity of testing the spectroscope on stellar or nebular spectra; but they have tried it on solar and electric ones, and consider that it will be very effective, from the solidity of its structure, and its large dispersion combined with a small deviation.

8. The balance of the grant, which will probably be absorbed by the expense of packing the instrument for removal, was not sufficient to justify the Committee in ordering a photographic apparatus which should be worthy of the Telescope; but some trials have been made with a "makeshift affair," which confirm them in their opinion that it is most important to turn to account the photographic power of this magnificent instrument, not only for the moon, but for the planets *and the sun*. On two or three occasions Castor and the moon were taken with this temporary apparatus; as to the star, its components impressed their images in good measurable disks in times varying from two to eight seconds; with respect to the moon, on February 1, when she was seven days old, and the air good, a remarkably hard and sharp picture, full of minute details, was obtained, which exhibits so strongly the great photographic power of

* The cause of this diffused light has since been discovered and removed.

† On the 19th of February, however, the Committee had the advantage of the presence of Mr. Lassell in the examination of Uranus among other objects with Speculum A, and, guided by his familiarity with that object, they were enabled to make out stars the positions of which, with great probability, corresponded to the places of the two most distant, and one of the nearest satellites.

the instrument, that they would regard it as a serious loss to science if this was not most fully brought into action. This is the more desirable because lunar and solar photography would utilize a considerable portion of time, during which the observing of nebulæ is impossible. After full inquiry, the Committee find that the cost of the necessary apparatus for this work, including a micrometer for measuring distances and positions on the photographs (like that described in the Philosophical Transactions, 1862, p. 373), could be provided for a sum not exceeding £400; and they earnestly hope that so valuable an addition may be made. They have been given to understand that the Melbourne Government have resolved on putting a roof over the instrument, but that they think it can be more economically constructed there than here. In this case it occurs to the Committee that the second form of roof described in Dr. Robinson's letter might be preferable to the more complex one there recommended; and as it would be less costly, the difference would more than cover the expense of the photographic apparatus. In this hope they have directed Mr. Grubb to prepare a detailed plan of that apparatus and of the second form of roof. The Committee conclude by stating that they have no hesitation in declaring that the instrument is perfectly fit for the work for which it was destined. They therefore consider that Mr. Grubb has fulfilled his contract, and have directed him to lose no time in preparing the necessary cases and packing it for Melbourne. They have also instructed him to insure it against the risk of fire during its stay.

The Committee feel bound to say that Mr. Grubb has put a most liberal construction on the terms of his contract; and after their minute examination of the excellence of the Telescope, and the amount and perfection of the machinery connected with it and its manufacture, they are convinced that Mr. Grubb has been more influenced by the desire of producing a perfect instrument than by any prospect of pecuniary advantage, and can scarcely realize the possibility of giving so much for the sum named in the contract, especially when it is considered that special works had to be erected for the purpose of constructing the telescope.

Rosse,
T. R. Robinson, D.D.,
Warren De La Rue.

Feb. 19, 1868.

P.S. March 7th, 1868.—I would strongly recommend that the photographic apparatus should be fitted to the telescope before it leaves Ireland.

Warren De La Rue.

XII. *Separate Report from Mr. Lassell to General Sabine.*

Ray Lodge, Maidenhead,
February 28, 1868.

My dear Sir,—I have ventured to think that, having had an

opportunity of examining the mounting and appliances of the Melbourne Telescope by day, and its optical performance by night, you might not be unwilling to receive from me some report of what I think of its performance and construction, even though that report be quite voluntary and unofficial.

I may express, first, my great admiration of the general efficiency and completeness of the mounting and driving-motion, in which the difficulties of meeting the requirements of firmness of position, and yet of freedom and convenience of motion, are overcome more perfectly than I had ventured even to anticipate.

Some photographs of the moon when about a quarter old were shown to me, which, though taken only in an experimental and preliminary way, I thought quite as good in relief and sharpness as any I have seen; at least, that is my impression, without the advantage of actual comparison.

The only opportunity I have had of trying the optical power and general performance of the Telescope was the night of the 19th inst., which, although cloudless, was obviously quite unfit to test the powers of any Telescope of large aperture. A *critical* examination of the figure was therefore impossible; yet I saw enough to convince me that there could be no great error of figure; and I had no *proof* that there was any which was appreciable. Some of the objects we saw were the Nebula in Orion, γ^2 Andromedæ, 46 and 51 Messier and Uranus. The two latter, which had the greatest altitude, were best shown. Under the circumstances, I considered the vision highly satisfactory, and I should anticipate that in a fine and tranquil state of atmosphere the performance will justify very sanguine expectations. It is true that none of the objects were as well shown as, on some occasions, I have seen them in the Telescope I had at Malta; but I think it highly probable that the superiority of altitude and atmosphere may fully make up the difference.

Bad as the circumstances were, Uranus appeared accompanied by what I believe to have been the two brighter satellites, which were indeed more conspicuous than I expected to see them; and although my own vision did not suffice to show me any points within these two, a star was seen by at least two observers, at such a distance as makes it probable that it was really one of the interior faint ones, which certainly I should not expect to see on such a night.

"In conclusion, I freely express my opinion that the entire instrument is a great triumph of mechanical engineering and optical skill; and, with the advantages of efficient working and a fine atmosphere, I trust it will still add something to our knowledge of the heavenly bodies.

I remain, my dear Sir,
Very truly yours,
WILLIAM LASSELL.

To General Sabine, R.A.,
President of the Royal Society, &c. &c. &c.

At the same Meeting it was

Resolved,—That the report now read be received and adopted, and that it be communicated to the Society, together with Mr. Lassell's letter, for publication in the Proceedings; also that Mr. Grubb be authorized to proceed without delay in the construction of the Photographic Apparatus referred to in the Report.

XIII. *From Mr. Ellery to General Sir Edward Sabine.*

Observatory, Melbourne,
1869, Jan. 4.

My dear Sir,

The telescope has at length arrived, and we are now very busy getting it erected, for nothing could be done towards it till the great machine itself came to hand. It will be nearly two months before it can be fairly tried, when a spacious rectangular building and its travelling roof will be completed.

Mr. Le Sueur arrived nearly two months before the telescope, having come by the overland mail, and the ship carrying the telescope making an unusually long passage.

The principal or more delicate portions of the instrument came out in good order; the specula are still in thin coats of varnish, and their surfaces appear in perfect good order. Some of the large castings and portions of the gearing had got rusted, but not to an injurious extent. The piers were completed on new year's morning, and form a magnificent piece of masonry, the stone employed being the grey basalt so common here (called "blue stone"), in blocks of from one to three tons in weight each. The building we have finally decided upon is of stuccoed brick-work, 80 feet long by 40 wide. Forty in length is taken up by the telescope-room, which is covered by a ridged roof of iron travelling on rails on the walls, and moves back on the outer 40 feet of building, leaving the telescope in the open air. The back 40 feet is covered by a fixed roof lower than the moveable one, and will contain a polishing and engine-room, a capacious laboratory, and an office for observer. The cost of piers, building, and roof will be about £1700. The Government, with hard economy in all other directions, have still acted very liberally about this work, and I only trust the telescope itself will turn out all that is expected of it. The micrometer and spectrum apparatus have not arrived yet.

I remain,
My dear Sir,
Yours faithfully,
Robert L. J. Ellery.

General Sabine,
Royal Society, London.

XIV. *From Mr. Ellery to the Astronomer Royal.*

(Extract.)

Melbourne Observatory,
August 13, 1869.

The great reflector is erected and at work on the Southern Nebulæ. This morning Mr. Le Sueur picked up Winnecke's comet for the first time, for we have had a long succession of bad weather.

I am not satisfied with the performance of this great instrument, and I do not think the mirrors are at all perfect in figure; I have not yet seen anything like good definition of a planet with the lowest power. Of course we get plenty of light, even with the nebulæ, but no definition with bright objects. The mechanism performs well, and the clock works very satisfactorily for so heavy a mass as it has to move. The 8 tons of moving parts are as easily handled, as far as setting and sweeping is concerned, as much smaller instruments.

(Copy.)

XV. *Rev. Dr. Robinson to Mr. De La Rue.*

Observatory, Armagh,
October 28, 1869.

My dear Sir,

Like you, I am much surprised that Mr. Ellery complains of the definition of the great Melbourne telescope.

It is fortunate that Mr. Grubb put in his contract that the telescope should be tried by the Committee before he sent it from Dublin, and our Report is sufficient to prove that in *our* hands it performed admirably. How it behaves under other management may be a question; and before adopting Mr. Ellery's opinion, that a mirror which in Dublin fully divided γ^2 Andromedæ does not define well at Melbourne, I would suggest that there may be other causes for this than any fault of the maker or of the Committee who passed it.

I. The speculum A (which I believe is now in the telescope) was never touched after you saw it on February 19. Speculum B was repolished to remove a slight diffusion of light which you noticed, and which was perfectly removed by the process. Mr. Grubb's command of figure is so great that I am certain this speculum also was perfect when it left his hands*.

II. It is possible that the great and small mirrors are not in perfect adjustment. This adjustment is easily performed; but from the construction of the Cassegrain and Gregorian, any want of it is far more sensible than in the Newtonian.

III. The film of varnish, if not removed, will, of course, be injurious. Mr. Le Sueur, in a letter to me some months ago, mentioned its existence, to which I replied, that if he had used strong and pure alcohol he would have found no difficulty in removing it. In

* This paragraph is an answer to an inquiry by Mr. De La Rue.

a letter to Mr. Howard Grubb he described the film as "sticky." Lac, as far as I know, is not viscid when dry, and therefore, I suppose, that the alcohol which he used was too weak.

IV. In the same letter to me he mentioned that he had been altering the position of the eyestops of the eyepieces, which is very likely to have interfered with the performance of the instrument.

In my reply I repeated to him in full the proper mode of adjusting the eyestop, and recommended him to replace it in its original position, and at the same time detailed the mode of adjusting the mirrors.

V. It is possible that they may have screwed up too far the three great screws behind the speculum-box, or the screws of the side support, so as to have put the mirror in a state of strain.

VI. And, lastly, it is possible that Mr. Ellery, though a practised observer, may not have made sufficient allowance for the effect of atmospheric disturbance in a telescope of so large an aperture. If, as I think, he has not been in the habit of observing with anything larger than an 8-inch achromatic, he may not easily realize the fact that in the great Melbourne telescope the disturbing effect would be forty times greater.

Without the operation of some of these causes it is difficult to conceive how Mr. Ellery could find fault with this mirror until he has practised on a small mirror. If he expects to *improve* Mr. Grubb's figure he may be woefully disappointed.

As Sir Edward Sabine took so much interest in the great Melbourne telescope, I wish you would show him this letter.

My failing sight obliges me to employ an amanuensis.

Yours truly,

T. R. Robinson.

W. De La Rue, Esq., F.R.S.

In January 1870 a communication, dated Nov. 8, 1869, was received from Mr. Le Sueur giving an account of the removal of the telescope to Australia, and of the first observations made therewith. It is here reprinted from the 'Proceedings' of the Royal Society, vol. xviii.:—

A description of the great Melbourne reflector, and its history, up to the time of inspection by the Committee, have been communicated to the Royal Society; the following additional account of the doings connected therewith since the instrument was consigned to my care may be of interest to the Society.

Mr. Grubb commenced taking down the telescope at the end of April 1868; this was accomplished in no great length of time, and without any difficulty. The specula (by the advice of Mr. Lassell, who had found this method answer perfectly) were coated over with shellac varnish to prevent oxidation on the voyage out; they were then protected in their cells and on their lever supports by strong double wood casings, and the other parts of the telescope and ma-

chinery cased or otherwise protected. The only casualty which there seemed to be any reason to fear could give rise to any serious consequences was a tilting over of the speculum cases; their great weight was, perhaps, a sufficient guarantee from such an event: it was nevertheless thought prudent that the telescope, and machinery generally, should not be left entirely to the tender mercies of the shipping and crane labourers; I was therefore present at the shipping in Dublin on board a steam-tug hired for the purpose, and at the transshipment in Liverpool, on board the 'Empress of the Seas.'

Both these operations were performed satisfactorily, and without any serious casualty.

The 'Empress of the Seas' sailed from Liverpool on the 17th or 18th of July; I followed by the August Overland Mail.

On my arrival in Melbourne I found that, beyond the selection of a site in the Observatory grounds, nothing had yet been done towards the erection of piers or building; this was principally owing to the fact that Mr. Ellery and the Board of Visitors had not considered the information which they possessed sufficiently definite to warrant their placing the matter in the hands of the Works Department; it had therefore been thought advisable to await my arrival.

Some necessary modifications having been made in the drawings, the construction of the piers was soon proceeded with, and satisfactorily terminated at the beginning of this year.

In the mean time the 'Empress of the Seas,' with her precious cargo, had arrived, after a very long voyage, which for some time was the cause of much uneasiness; parts of the instrument were unpacked and temporarily housed: the whole appeared in fair order; there was certainly no material damage done to anything.

Arrangements being in progress for the erection of a suitable building, it was thought advisable to delay mounting the telescope until part of the building was constructed; little therefore was done for some time beyond setting up, as accurately as possible, the plummer-blocks which contain the polar axis bearings.

The building was commenced early in the year, and when it was thought that sufficient progress had been made, the crane which had been used in the erection of the piers was removed to a more convenient position, and the various heavy parts of the instrument lifted on to the floor of the telescope-room, over the walls or through a gap left for that purpose, and, for convenience in after operations, in the north wall and north end of west wall.

The mounting was then proceeded with, and satisfactorily accomplished in little more than a week, as regards the main parts, without much difficulty.

Attempts were made on two occasions to use the instrument for adjustment and observation, but it was found that the dust (a dreadful enemy in the summer) and the grit caused by the building, accumulated to such an extent as to lead to fear of considerable damage to the bearings and more delicate parts of the machinery; it was therefore deemed prudent to cover up the telescope as well as possible with tarpaulins, and leave it in that state for some time.

The building is rectangular, 80 feet long meridionally by 25 wide, with walls 11 feet high. Of the meridional length the telescope-room occupies the north 40 feet; the next 12 feet are appropriated to the polishing-machine, crane, and engine; the remaining 28 feet are divided into two rooms, one of which is at present used as an office, the other, 25 by 14, is intended for a laboratory. The moveable roof is 40 feet long, and runs on rails laid the whole length of the walls; the telescope-room may therefore be completely covered in, and as completely uncovered when required, the roof in the latter case resting on the south building, which on that account has a very low permanent roof.

The roof is constructed of six triangular wrought-iron principals, cross-braced, which abut at each side on a broad horizontal plate formed of two parallel lengths of stout angle-iron, connected at various points by iron bands; for additional strength, a broad vertical plate is bolted to the outer angle-iron piece. There are four pairs of wheels, 26 inches in diameter, flanched on the inside; these lie along the middle of the horizontal plate, the journals being bolted to the angle-iron pieces which form the plate.

The roof is covered with galvanized corrugated iron; it is therefore on the whole a somewhat heavy affair. The mechanical arrangements for moving are, however, simple and effective; a stout iron shaft runs across the building, and gears by wheel and pinion on the axles of the two south end wheels; to this shaft is fixed a spoked hand-wheel, by means of which the operator readily sets the roof in motion, and standing on a small platform connected therewith, is himself carried along at the same time.

The design of the roof is due to Mr. Merrett, of the Works department. On the whole, there is much to be said in favour of this rectangular form of roof: the temperature even in this climate frequently descends too low to be pleasant; but the occasional bodily inconvenience produced thereby is more than counterbalanced by perfect freedom to the observer, and the gratification of knowing that the instrument is in the best possible conditions for satisfactory performance. Only one really serious annoyance have I found connected with complete exposure; I allude to occasional heavy dew rendering it almost impracticable to use the sketching and other papers, the speculum meanwhile remaining free from deposit if precaution is taken not to work at too great an altitude.

The telescope, when housed, lies meridionally on the east side of the pier, and nearly in a horizontal direction, provision having been made to prevent the tube being lowered beyond a certain small inclination.

The piers are in keeping with the massiveness of the instrument; they are constructed of large, not to say huge blocks of basalt axed to a fine surface, altogether a substantial and beautiful piece of work.

The height of the walls with reference to the piers is such that very little of the sky range is curtailed. The north wall cuts off objects having a lesser altitude than about 10°. When resting on

the east or west walls the telescope is nearly horizontal; in both these directions trees interfere, especially on the west side, where the ground rises. This curtailment will probably be a matter of very small importance, as, with a four-feet aperture, observations at low altitudes are almost impracticable, and would probably never have to be resorted to, except in the case of comets. The roof itself cuts off some of the range near the subpolar meridian; this, again, is not likely to be of much consequence.

The steam-engine, polishing-machine, and crane have been mounted in the room devoted to them; this room adjoins and is on the same floor (raised 4 feet from the ground, and 3 to 6 feet from the floor of the other rooms) as the telescope-room. To the east end of this machine-room, and communicating therewith, a small lean-to boiler-house has been added; in the west wall is a window which, when open, will leave sufficient clear space to admit of viewing a distant nearly horizontal object for the purpose of testing the mirrors.

The large speculum (A) was originally attached to the tube in its varnished condition; on the first favourable occasion it was taken down and unvarnished—a process which proved more troublesome than had been anticipated. The lac was very refractory, and the difficulty of removal exaggerated by the extreme heat then prevalent; after a process of solution in alcohol, mopping up, and washing with water frequently repeated, although there seemed no lac which would still dissolve, a large number of markings caused originally by the varnish brush were apparent, and the whole surface had un unpleasant mealy appearance.

It was thought, however, that the light lost would not prove serious, and in any case it did not seem that any further operation except polishing would improve matters; the speculum was therefore remounted and tried; and although it was of course impossible to say what would have been the effect of a more perfect polish, the views given of the brighter nebulæ were grand in the extreme, and left nothing to be desired.

By degrees, however, and without much exposure, the surface became more and more tarnished, with evident effect on the performance.

In the meantime the second mirror (B) had been unvarnished; in this case naphtha was used as the solvent, the solution mopped up, and the surface washed with soap and water. After a frequent repetition of this process, the surface seemed clear of impurities, and though not so bright as I had frequently seen it in Mr. Grubb's workshop, there were no signs of mealiness, the only unpleasant casualty being a considerable pitting of two patches some two inches square, produced by droppings from the muriate used in soldering the tin cover. These pittings are deep and unsightly; but the extent of surface corroded is comparatively so small that the effect must be inconsiderable.

The specula were exchanged about two months ago, and A put on the machine; but nothing has yet been done towards repolishing, as the necessary arrangements have not been got together for performing that delicate operation with due convenience.

Of work done, I cannot yet speak with any satisfaction since it became at all practicable to use the telescope; the history which I have to relate is a long chapter of weary heart-breaking watchings, with an occasional half hour's work.

η Argo was the first object observed for purpose of delineation; after the first night's work little (and that by snatches) was done towards it, the lapse of time caused by a new inroad of workmen and a long course of extremely unfavourable weather having carried the nebula out of convenient reach. The search, which was reluctantly given up, will, however, be again soon resumed.

I enclose two sketches, 4403 and 3570, of the 1864 catalogue.

4403. The horseshoe nebula is a grand object, conspicuous and with shape even in the finder. In the sketch the principal stars are laid down from measured position-angles about different centres; they are not as accurate as I could wish, and will be reobserved differently under better conditions; in no case, however, can there be sufficient error to influence in any material degree the configurations of the nebula or the smaller stars sketched in by eye.

It will be seen that the sketch contains considerably more detail than the corresponding figure in Herschel's Catalogue; there appears, however, to be no marked difference (with perhaps one exception) which may not be accounted for by the difference of aperture used.

The exception to which I allude is the presence of a small but conspicuous double star at the s. p. angle of the knot which lies between the γ and the bright streak; the experiment has not been tried of cutting down the aperture to approximate to an 18-inch Herschelian, but the intrinsic brightness of the principal star, and the presence in the C.G.H. of stars not more bright (No. 3 of Herschel's catalogue is certainly less bright) go far to show, without this experiment, that the star did not exist as such with its present brilliancy at the time of the C.G.H. and P.T. 33 observations .... I have not seen Mr. Mason's drawing, but look forward with much interest to examining it and his remarks thereon.

The important position of the star, and the careful scrutiny which the knot and its neighbourhood must have repeatedly undergone, forbid the assumption that it was simply overlooked by Sir John Herschel.

The star β (I keep to Sir John Herschel's numbers and letters) is conspicuously and beautifully double, the companion of considerable brilliancy, about 15 mag.; with its present brilliancy and elongation it should, I think, be within reach of an 18-inch.

The knot is what I presume should be called resolvable; the appearance is sparkling, though no discrete stars can be seen, except perhaps a second faint one, which is suspected at the s. f. angle. Part of the streak near to the knot is also sparkling, but not in so marked a manner; the other portions appear of the ordinary milky nebulosity.

The fainter nebulosity (S) of the bright streak pretty well marks out the borders of the almost vacuous lane which leads up to and

past the knot. On receding from the lane it becomes very faint; nor is this faintness uniform; but the appearances are so fugitive that, after repeated and painful effort, I have been unable to catch them; the borders, however, stretching to the stars, as in the figure, are occasionally pretty well seen. On one or two occasions I have suspected the existence of a link between the nebulosity about the star No. 10 and the lower portion of the ♋; this, however, requires verification.

At the f. end the upper and smaller semicircle is plainly marked, the lower and larger very faint, and consequently its exact figure uncertain; there is certainly some very faint nebulosity leading through the groups of stars north of the three bright f. end stars, but it has not been added to the sketch on account of its uncertain figure and extreme faintness.

3570. A small but beautiful spiral. The two brighter knots are resolvable; the greater brightness of these knots is not particularly shown in Sir John Herschel's sketch, but is mentioned in the observations; the general ground is only slightly nebulous.

Of work out of the regular course, amongst other things, Neptune has been observed on some five or six occasions for figure and a second satellite, with only negative results.

In the absence of a photographic apparatus to be used at the uninterrupted focus of large mirror, attempts have been made to utilize the 2nd or Cassegrain image; an average exposure of near ten minutes on an eight-day moon produced pictures which (by no means good) were of sufficient promise to make it worth while to resume the attempt under more favourable conditions.

The time of exposure is somewhat surprising, and would seem to accuse a great loss of chemical rays by a second perpendicular reflection; but perhaps the more legitimate conclusion would be that the inactivity was mainly due to absorption at the surface of the large mirror, which was then very yellow.

The spectroscope arrived some time ago, but has not been much used; it is thought that for *star* work of any value some modification will be required, principally the exchange of the present collimator for one of longer focal length. A greater dispersion, moreover, seems desirable; for nebular work, however, for which it was mainly designed, the spectroscope in its present form, which is handy and compact, will be of much service.

For spectroscopic work on objects having a sensible diameter, the great telescope itself labours under some disadvantages; the enormous focal length and consequent magnification of the image is a serious inconvenience in the case of faint objects, and may be only partially remedied by a suitable condenser. This magnifying of the image may, however, in some cases be advantageous: I allude to the possibility thereby afforded of viewing small definite portions of moderately bright objects; unfortunately the objects with which we have to deal are seldom of such a character.

Of nebulæ, Orion has been examined for purpose of practice. The three lines are plainly and conspicuously seen; the hydrogen

line is comparatively much fainter than I had anticipated, and disappears in the fainter portions of the nebula. 30 Dorado shows the nitrogen line with facility; the second line certainly, but not in all positions, and always with difficulty; the hydrogen line is suspected only. I can see no trace of a continuous spectrum.

η Argo has been observed on only one unfavourable morning; the nitrogen line was seen over a considerable space; of the presence or absence of others, or of a continuous spectrum, I am unable to speak with certainty.

With respect to future operations, it is intended that at first the routine work shall consist of a detailed delineation of the objects figured by Sir John Herschel, or any others which may prove interesting: this will take some time; for even without the impediment of cloudy weather, the delineation, with any degree of satisfactory correctness, of a moderately large nebula requires a considerable amount of work and careful and frequent scrutiny. It is hoped, however, that this work will by practice be found less painfully difficult than it is at present.

The spectroscope will be used as much as possible, the moon photographed, and attempts made to photograph the nebulæ, when a photographic apparatus has been procured, and staging, photographic room, &c. added to the building. It is, moreover, hoped that before long a refractor, of some nine inches aperture, may be procured, to be mounted with the reflector, or, preferably, as a separate instrument.

This telescope, besides being of much general use, will find much and valuable employment in determining micrometrically the chief points in the nebulæ under examination with the reflector, with more expedition and accuracy than at present; for spectroscopic work this telescope would be a valuable adjunct, especially if it be constructed of such comparatively short focal length as seems now to be practicable.

The great interest which the Royal Society have taken in everything connected with the Melbourne reflector is my sole apology for sending thus early such a meagre account.

XVI. *Mr. Verdon to Sir Edward Sabine.*

8 Victoria Chambers,
Victoria Street, Westminster, S.W.
February 22, 1870.

Sir,

I am directed to express the thanks of the Government of Victoria to you, and to request that you will be pleased to convey them to the Telescope Committee, for the very valuable advice, assistance, and supervision given by you and them to the construction of the great Melbourne telescope.

The instrument is now engaged upon the work for which it was intended; and the Government of Victoria looks forward

to the important astronomical results which it will doubtless achieve.

I am further directed to communicate to the Royal Society a report by the Government Astronomer, upon the working of the Telescope, a copy of which I beg leave to inclose accordingly.

Referring to the requisite appliances for photographic work, mentioned in Mr. Ellery's report, the Chief Secretary observes that whatever sum may be necessary for the construction of the apparatus will be very gladly provided by the Government.

I have the honour to be,
Sir,
Your most obedient Servant,
GEO. VERDON,
Agent-General for Victoria.

Lieut.-General Sir E. Sabine, K.C.B., &c., &c,
President of the Royal Society.

XVII. *Mr. Ellery to Sir Edward Sabine.*

Melbourne Observatory,
January 3, 1870.

MY DEAR SIR,

* * * In a letter to Mr. Airy I mentioned the bad definition of the large mirror (believed to be that known as A), which was then in the telescope. Under the most favourable circumstances, I never once obtained with it what I could call *moderately good definition.* I tried it on Saturn many times, but could not get any detail whatever. The larger division in the ring was visible, but was never distinct; beyond this nothing on the ring or the planet itself could be made out. I have, however, never had any experience in observing with large reflectors, nor with refractors larger than 8 inches or so, and therefore may not have made all the allowances I should have done; but after over twenty years' constant observing with telescopes of various kinds, my statement that the definition with mirror A (?) was not good, may be entitled to some weight; and when this statement is endorsed by Mr. Le Sueur, who has very recently observed with large reflectors in England and Ireland, the Committee may be assured that the statement was not made with any desire to find fault, or to convey anything but the actual truth of the case. The mirror A (?) at first had a mealy yellow appearance after the lac had been removed, which was thought to be due to a little residual varnish. This dull mealiness increased, till eventually Mr. Le Sueur was obliged to change it for B (?). The telescope performance is very much improved with B (?), and on several occasions I have obtained views of Jupiter and multiple stars, when the definition has been excellent.

The metal in mirror A, on microscopic examination, I found not to be homogeneous, the surface being as it were reticulated. Masses

of good alloy, bearing a fine polish, are intersected by reticulations of a yellow alloy, which appears somewhat porous, and from which all the oxidation seems to arise. No such state of metal is presented by mirror B, which is now in use.

As far as regards the mechanical arrangements, the telescope is in capital order.

I have given to our Government a brief report of the erection of, and subsequent work with, the telescope, a copy of which will, I believe, be sent to the Committee.

Mr. Le Sueur has been fully occupied during every available night with nebular work. A few nights since he observed the spectrum of η Argus, and found it to contain bright lines, C and F (hydrogen) certainly, others not so distinct, probably part of the nitrogen spectrum. This is a most interesting discovery with respect to a star with such a large variability. Mr. Le Sueur sends these observations to Mr. Stokes by this mail.

* * * * * *

I remain,
My dear Sir,
Yours faithfully,
ROBT. L. J. ELLERY.

Sir Ed. Sabine, Royal Society, London.

XVIII. *From Chief Secretary MacPherson to Mr. Verdon, inclosing Report from the Government Astronomer at Melbourne.*

(Copy.)

THE GREAT MELBOURNE TELESCOPE.

Victoria, Chief Secretary's Office,
Melbourne, Jan. 3, 1870.

SIR,—The great Melbourne telescope having now been in fair working order some three or four months, it appears to be a fitting time to request that you will convey to the Telescope Committee of the Royal Society, through the President, General Sabine, the thanks of this Government for their very valuable advice, assistance, and supervision in the construction of the instrument which is now engaged upon the work for which it was intended, and, it is hoped, is destined to achieve important astronomical results.

You will be good enough to communicate to the Royal Society the inclosed report of the Government Astronomer, which, although in some respects not so satisfactory as could have been desired, will be read with much interest. It is unnecessary for me to say that the Government will make provision for meeting the cost of the photographic appliances, to which reference is made.

I have, &c. &c.
(Signed) JOHN MACPHERSON.

George F. Verdon, Esq., C.B.,
Agent-General, London.

(Copy.)

Observatory, Dec. 31, 1869.

Sir,—I have the honour to submit for your information a report of the erection and subsequent working of the great Melbourne telescope. The telescope arrived in November 1868, and its erection at the Observatory was commenced early in January of the present year. The building for its protection was not completed till June, and the final fitting up of the telescope finished early in July.

From a report furnished by Mr. Le Sueur (the astronomer in charge) I extract the following:—"Observations were attempted as early as April in this year, but the telescope was not in fair working order till about the middle of August, since which date observation has progressed more or less satisfactorily.

"The principal work has consisted in examination and mapping out of nebulæ; but the very unfavourable weather throughout almost the whole season has prevented much progress.

"Of other work, position-observations have been made of Winnecke's periodical comet; except in so far that largeness of aperture may permit the following up of a comet to a late date, the great telescope is, of course, unsuited to that kind of work, and bad weather at the critical time prevented a full utilization of the above-mentioned peculiarity; yet it is thought that the later observations may be of value in filling in the series which will have been obtained in Europe.

"When favourably situated, Neptune was observed on many occasions, for examination of figure and detection of new satellite, with negative results in both cases."

With respect to spectroscopic observations Mr. Le Sueur says:—"The spectroscope furnished by Mr. Grubb has already proved of much service for nebulæ work; for star work, as far as I can at present judge, it is unsuitable; nevertheless, a very important observation has lately been made therewith, showing that the spectrum of the variable η Argo is crossed with bright lines. To follow up this, and star-observation generally, to much purpose would require a considerably modified spectroscope. The one now on hand was intended specially for nebulæ work, and for this it is as competent as could be desired."

Nothing has yet been done in celestial photography. It was supposed that the original vote would have been sufficient to cover the expense of a suitable photographic apparatus, but the many extra expenses incident to packing, shipment, &c. exhausted the available funds. The Royal Society of London, however, deeming it of great importance that the telescope should be fitted for photographic work, kindly took it upon themselves to order the requisite appliances, which I believe are now in course of construction by Mr. Grubb (the maker of the telescope).

We are not yet informed of the exact cost of the apparatus, but it will be necessary to relieve the Royal Society of its self-imposed burden.

In my report to the Board of Visitors in April last, I referred to the

construction of the telescope, but could not at that time speak of its performance or capabilities; but during the several months' use since then, Mr. Le Sueur has tested its performance most carefully; and although I have had no prior experience with reflecting telescopes of such dimensions as this, I have been enabled from frequent observations to form an opinion of its powers.

With the large mirror first used the telescope certainly did not perform so satisfactorily as could be desired, and, making all allowances for atmospheric disturbances, the definition was never good; but with the other mirror (supposed B) it became very much better, and Mr. Le Sueur speaks of its performance now as "far more satisfactory." The large mirror first in use has become very dull, from a peculiar kind of yellow tarnish, and it is highly probable that it will have to be repolished before it is fit for much service; and considering the great delicacy of the operation, and necessity of considerable experience in its performance, it will be advisable to obtain a supplementary metal mirror of 12 or 15 inches diameter, which should be figured and polished, not only for the practice of the astronomer in charge of the telescope, but also for the training of the assisting workmen.

The building for the protection of the telescope is in most respects satisfactory. The peculiar construction of the roof renders the exclusion of dust a difficult matter; but much, I believe, can yet be done to improve it on this score. The rolling gear, by means of which the roof is traversed from over the telescope, was somewhat defective at first, but has since been made to work satisfactorily. The polishing apparatus, with its driving steam-engine, are in their places, and ready for use whenever they may be required.

The necessary stages for testing the mirrors when on the polishing machine are now in course of erection. A photographic room has been erected in the telescope-room, and will be ready for use in a few weeks.

There are several arrangements and appliances yet required before the telescope can be said to be properly provided, among which are more convenient observing-seats, drawing-stands, and the erection of a platform outside the telescope-room for photographic operations. Some of these are already in progress, and, should sufficient means be available, will soon be completed.

I have, &c. &c.,

(Signed) Robert L. J. Ellery,
Government Astronomer, &c.

The Hon. the Commissioners of Crown Lands and Survey.

XIX. *Rev. Dr. Robinson to Sir Edward Sabine.*

March, 1870.

My dear Sir Edward,

I have read with pleasure the copies of letters from Mr. Verdon and Mr. MacPherson, which you have sent me, on the subject of

the great Melbourne telescope, and the accompanying report by Mr. Ellery.

The former acknowledges in most gratifying terms the share which the noble body over which you preside had in carrying out the grand scientific conception of the Victoria Government.

Mr. Ellery's report is not less important; for it shows that the difficulties and objections which always beset a great and novel undertaking are clearing away, and a bright and prosperous view is opening of the future.

Of the objections, which were numerous, many have been circulated in the Melbourne papers, and some have been communicated, not only officially to me and Mr. Grubb, but also to others in this country, through whom they have gained publicity.

(i.) One of the complaints referred to in Mr. Ellery's report is that a proper observing-chair was not furnished along with the telescope. The Committee requested Mr. Le Sueur, when in London, to examine the most approved machines for the purpose, and gave him *carte blanche* to order that which appeared to him the most suitable. He selected the Dawes chair; and if it has failed in its object, he is the only person responsible for the failure.

(ii.) Mr. Le Sueur thought it necessary to alter the eyestops of the eyepieces from their normal position, influenced by the hope of a little increase to the field of view, but at the cost of more than equivalent loss of light.

(iii.) Mr. Le Sueur complained that the spectroscope which was sent out was not adapted for all spectroscopic uses, and that the Cassegrain telescope laboured under some disadvantages on account of the great size of its secondary image.

The objection derived from the large size of the image may be removed by condensing the image by a convex lens, either spherical or cylindrical. The limit to which this can be done is determined by the condition that the vergency of the condensed pencil shall not exceed that of the collimator's object-glass. In the case before us the vergency of the rays of the second image $=\frac{1}{41}$, and that of the collimator $=\frac{1}{4\frac{1}{1}}$; so that the image can be condensed to $\frac{1}{10}$ of its diameter, and its brightness increased a hundredfold.

As Mr. Le Sueur has lately been doing very good work with this spectroscope on nebulæ and planets, he has probably in some measure surmounted his difficulties; and that which he still seems to feel about its application to stars will similarly disappear by the use of the cylindrical condenser.

When this spectroscope was constructed, it was not supposed that a high dispersive power was available for nebulæ; and the highest for which it was furnished did not give more than 5·5 degrees of dispersion from B to G. A compound prism of Mr. Chance's new dense-glass can be furnished at a moderate cost, which, without any alteration of the spectroscope, will give double the dispersive power, probably sufficient for everything except the sun; and I have directed Mr. Grubb to send one out with the photographic apparatus, which is now nearly completed.

(iv.) Mr. Le Sueur complained that the micrometer's field of view is inconveniently small. The size of the micrometer was, however, decided on after a careful discussion, in every step of which he took part, and to which he appeared to assent. An ordinary micrometer with an illuminated field is quite inadmissible for nebular work, and the only alternatives were one with wires illuminated in a dark field, or a double-image one. I had a strong inclination to try the latter, of which an elegant form was long since described in the *Philosophical Transactions* by Ramsden, in which the small mirror of the Cassegrain is divided, and its segments are separated, as in a heliometer. This, however, has the great disadvantage of halving the light of the images, and further lessening it by requiring a diaphragm to be placed behind the small mirror twice its diameter.

In the illuminated wire micrometer the vision of nebulæ is least interfered with; but the apparatus required for its illumination is bulky, and, if it had been applied to the lowest power, would have attained unmanageable dimensions.

It was therefore resolved on to make it of a size which would be sufficient to determine with accuracy the places of the stars of reference, and to complete the mapping of the nebulæ by means of glass gratings, which, on account of the great equivalent focal length of the telescope, admit of much accuracy.

In reference to a doubt which has been expressed by Mr. Le Sueur in regard to the figures of speculum A and the small mirror, which he considered were jointly affected with spherical aberration, I may remark that he had himself seen many difficult test-objects with that speculum in Dublin; in particular he was present when the Committee tried it, assisted by Mr. Lassell. It was then remarkable for the clean, sharp disks which it gave to large stars. And as speculum A does not appear to have undergone any process since it left Dublin which could alter its figure, the only possible mode of accounting for the aberration, if it really existed, is that he used the small speculum, which he says he repolished after its arrival at Melbourne, probably without paying attention to its figure.

As to the strange deterioration of speculum A's surface, I do not think Mr. Ellery's explanation of it in his letter to you is probable; namely, that it is composed of a yellow alloy disseminated through masses of good alloy bearing a fine polish; I have never seen any occurrence of the kind, and it is very unlikely, as the alloy is of atomic proportions. It cannot have been caused by any action on the metal of the lac varnish with which the surface was covered. No such action was observed on speculum B, which was similarly protected. And as, fortunately, the piece which was cut out of the centre of speculum A had been preserved and polished, I requested Mr. Grubb, on hearing of the matter last August, to varnish the disk with lac varnish, such as was used with the large speculum, and I have lately examined it. The varnish can be perfectly removed, leaving no trace whatever on the surface; and I may add that there is not the slightest appearance of the yellow alloy suspected by Mr.

Ellery. It is exceedingly improbable that any chemical agents could be present in the hold of the ship likely to penetrate the varnish; and even if such had been present, it might be expected to have acted on speculum B as much as on A.

I learn from Mr. De La Rue that lac contains, in addition to the resinous matter of which it is chiefly composed (and which is easily soluble in alcohol), a certain proportion of a kind of wax, which is far less easily acted on by that reagent. If the process for removing it was not carefully conducted, this waxy matter would remain mottling the surface. Now I do not think the process was carefully conducted; for I am informed by Mr. Le Sueur that methylated spirit was used instead of pure alcohol; this does not seem to have been largely applied; and afterwards the surface was "mopped with water."

If Mr. Le Sueur had provided himself with a quantity of fine carded cotton and absolute alcohol, and, soaking rolls of the cotton in the spirit, had gone patiently and carefully over the surface, it seems very probable that he would have by these means obtained a white and brilliant surface, such as we have with the disk already mentioned *.

In any case, however, the use of water was quite improper.

If the yellow stain be tarnish, it might yet be removed by rubbing with slices of lemon, after it has been *thoroughly* ascertained that no lac can be present.

Should this expedient fail, there is no resource, of course, but to repolish it. But I hope this will not be attempted until Mr. Le Sueur has had a good deal of preliminary experience. As a means of acquiring this, I quite concur in the necessity of sending out for practice a disk of speculum metal of 12 or 13 inches diameter, as recommended in the report. But something more than this will be necessary. You may polish a speculum to the highest lustre, which yet will show you nothing at all. It may not be parabolic, may be polished in rings, or, though the general figure be correct, it may be covered with innumerable little elevations and hollows destructive of good vision.

The disk, therefore, should be mounted on its own special system of equilibrated levers, and tested in a telescope.

But the operator must keep in mind that even when he succeeds in making his speculum of 12 inches define perfectly well, still he will have much to learn before he can bring speculum A to anything like the perfection which it had in Dublin.

I will conclude this long letter with a few words, which I hope will be useful to the Melbourne astronomers.

It is very difficult for persons who are only conversant with telescopes of moderate size to form an idea of the excessive disturbing influence which circumstances of temperature exercise on instru-

* In the February Number of the Astronomical Society's 'Monthly Notices,' Mr. Lassell states that he has removed, after eight years, a covering of lac from his 2-feet speculum, and that its brilliancy is perfect. He, however, used *absolute alcohol*.

ments of such enormous aperture as this. Even in Ireland, where the annual range of the thermometer is but small, and the air is generally charged with moisture, the number of nights on which even a 3-feet reflector can be used with complete success is very small; and at Melbourne, where these meteorological circumstances appear to take a far wider range, the difficulties must be immensely increased.

It is also probable that the telescope's being enclosed under a roof may make the difference of temperature between the great speculum and the atmosphere greater at the commencement of the evening's operations than if it had been mounted in the open air. The roof should therefore be opened an hour or two before beginning to observe.

The line of work which this imposes on the astronomer is evidently to delineate the coarser and more obvious details on nights of bad definition, and to devote those rarer and more precious ones to examining the finer and more delicate characters of objects.

It also may not be unnecessary to point out the great importance of always attending to the adjustments of the specula and to the firmness of connexion between those specula and the tube. The adjustments are more complicated than those of the achromatic, and more likely to alter; but they are easily made, and any neglect of them tells powerfully against the performance of the instrument.

(Signed) T. R. Robinson.

XX. *Mr. De La Rue to Sir Edward Sabine.*

The Observatory,
Cranford, Middlesex, W.,
April 22, 1870.

My dear Sir Edward,

As regards the additional prism requisite to give more dispersive power and some additional lenses, I have no hesitation in recommending that these should be sent without delay to Melbourne.

I suppose that the functions of the Melbourne Telescope Committee have come to an end with the completion of the photographic apparatus, which was the last of the appliances they were to superintend; but I presume that, notwithstanding the termination of their work, a recommendation from the members of it would have weight with the Victoria Government. Assuming this to be the case, I do not hesitate to recommend that a 12-inch disk *ground* but not polished, grinder and polisher, tube *and cell*, on the plan of the large one, and one or two secondary specula, also a diagonal mirror, together with some convenient stand to enable Mr. Le Sueur to direct the telescope on to a star, should be sent to him; for it is most essential that he should be put in possession of the means of acquiring the art of figuring the specula of the great telescope.

I do not suppose that these things should be furnished without

the authority of Mr. Verdon; and altogether they may cost £100; but this sum is quite insignificant in relation to the object to be gained.

I am,
Yours most sincerely,
WARREN DE LA RUE.

ENCLOSURES.

Rev. Dr. Robinson to Sir E. Sabine, dated May 4, 1870.

I send a copy of Grubb's estimate for a trial Telescope for Melbourne, of which I send another to De La Rue; I think it very low, considering that it involves an entire telescope of considerable power, which, if not worked in the Melbourne Observatory, could very easily be disposed of when it had done its work with Le Sueur. I should not, however, venture to order it without your obtaining Mr. Verdon's opinion on the subject.

(Signed) T. R. ROBINSON.

The Observatory, Cranford, Middlesex, W.,
May 6, 1870.

MY DEAR SIR EDWARD,

I quite approve of Mr. Grubb's tender as to details and prices; the only thing that strikes me is the desirability of having small circles graduated coarsely on brass, simply for facilitating the finding of objects. This addition would not cost much, say £5 additional. I write almost in the same words to Dr. Robinson.

(Signed) WARREN DE LA RUE.

The amount of the estimate was £165.

XXI. *Sir Edward Sabine to G. F. Verdon, Esq., C.B., Agent-General, &c.*

The Royal Society, Burlington House,
May 7, 1870.

MY DEAR SIR,

I enclose Mr. Grubb's estimate of the cost of the small reflecting Telescope proposed to be supplied to the Melbourne Government as an addition to the Great Reflector now at work; together with notes from Dr. Robinson and Mr. De La Rue approving of the same. On receiving your sanction, Mr. Grubb will at once proceed.

(Signed) EDWARD SABINE.

XXII. *Rev. Dr. Robinson to Sir Edward Sabine.*

Observatory, Armagh,
May 15, 1870.

My dear Friend,

The circumstances which induced Mr. De La Rue and me to recommend the increased outlay to which Mr. Verdon refers are these. In Mr. Ellery's report it is recommended that a disk of speculum-metal should be supplied, on which experiments should be made, for the purpose of giving the Melbourne astronomers that experience in polishing specula without which any attempt to repolish the large speculum would probably be a failure.

It occurred to us, however, that it is essential to the success of such experiments that the operators should have the means of ascertaining how far their trials are successful; for it is possible to polish a speculum to a high degree of brilliancy which yet, from defects of its figure, will perform badly in a telescope. Opticians try this by making the speculum into a temporary telescope and examining a watch-dial, or an artificial star placed at some distance; but this test is only available to a certain extent; and when the highest degree of accuracy is to be obtained (as is the case in this instance), recourse must ultimately be had to the observation of real stars. This requires that the observer should have the means of directing the experimental telescope to any part of the sky; it should therefore be provided with some sort of a stand. The speculum should be supported in a way exactly corresponding to that in which the great speculum is supported in its box, as otherwise the same proportion of action of the polishing-machine which gives a perfect parabolic figure to the small speculum, would not do so to the large one.

Now it appeared to us unwise to endanger these experiments by an economy which is not of importance. The cost of the proposed stand will be but little more than that of any one possessing the requisite steadiness; and the tube, as mentioned in the estimate, would undoubtedly be more effective than any makeshift which could be got up for the occasion. The lever-mounting is absolutely necessary.

For these reasons I hope Mr. Verdon will consider that these extra matters are essential to supplying the deficiency mentioned in Mr. Ellery's report.

Yours ever,
T. R. Robinson.

Sir Edward Sabine, K.C.B.

XXIII. *Mr. Verdon to Sir Edward Sabine.*

8 Victoria Chambers,
Victoria Street, Westminster, S.W.
May 18, 1870.

Sir,

Referring to your communications upon the subject of the Mel-

bourne telescope, and to the suggestions of the Committee, that the deficiencies mentioned in Mr. Ellery's report should be supplied at a cost not exceeding £165, I have the honour to inform you in reply to your inquiry, that in my opinion the Government of Victoria will sanction the expenditure proposed, in accordance with the assurance given by the Chief Secretary in his dispatch of the 3rd of January last, addressed to me, the substance of which I have already communicated to you.

I have the honour to be,
Sir,
Your most obedient Servant,
GEO. VERDON,
Agent-General for Victoria.

Lieut.-General Sir Edward Sabine, K.C.B., P.R.S., &c.
Burlington House, W.

Report of the Government Astronomer to the Board of Visitors to the Observatory, read at the Annual Visitation, March 31, 1870.

THE GREAT MELBOURNE REFLECTOR TELESCOPE.

At the date of my last Report I was unable to give any further account of this instrument than of its arrival, the workmanship, style of construction, and partial erection, which was not sufficiently advanced to admit of any test of its optical capabilities. I, however, mentioned several matters where some alterations and additions of no great importance had to be made; and also to the fact that it was fortunate we had not built the piers before the telescope arrived, in consequence of some error in the plan for them which was sent from Dublin*.

The principal alterations referred to are these:—

In consequence of the difference of latitude of Melbourne and Dublin, where the telescope was mounted during construction, the horizontal clock connecting-shaft had to be lengthened, the vertical shaft leading to R.A. sector shortened, and a new support made for it.

A new journal for the upper part of the shaft for quick right ascension motion—as the original one could not be kept in position owing to insufficient bearing.

As regards the piers—in the drawings sent to us by Mr. Grubb, the recess in the pier for the clock was too small by a considerable amount; and had the piers been built, consisting as they do of large blocks of finely cut bluestone, the south pier at least would have had to be taken down to rectify the error. The base of the piers, too, as shown in the drawings, would not have allowed the telescope to be

* It appears from Mr. Grubb's statement at page 5 of a pamphlet published by him in March 1870, that the tracing from a drawing purposely made for piers suited to the latitude of Melbourne, and which had been sent out "as instructions for the erection of the piers," had not been followed in their erection.

brought sufficiently near the vertical to admit of the removal or replacing the large mirrors, except at such an angle as would be at the least very inconvenient. And further, if the lower pier had been made according to drawing, its width would have prevented the telescope being brought to the meridian on either side.

Additions had to be made to the Finder Telescope, as no convenient means for adjusting it parallel to the large telescope had been provided. A new bearing, with centering screws, was therefore made to replace the original bearing, which admitted of no motion. It was also found necessary to put a focusing rack and an illuminating arrangement to the finder.

The final fittings of the telescope were not completed till early in July, as the building for its protection was not thoroughly finished until then. Observations were, however, attempted as early as April, but the instrument was not in fair working order till the middle of August, from which time observations have been carried on regularly.

The micrometer, which had not arrived at the time of the last Report, reached us in July, and the spectroscopic apparatus in September.

In a report on the state of the Great Telescope which I furnished to the Honourable the Commissioner of Crown Lands and Survey in December last, I referred to a photographic apparatus which I had incidentally learnt was ordered by the Telescope Committee of the Royal Society to be made. I stated, "It was supposed that the original vote would have been sufficient to cover the expense of a suitable photographic apparatus, but the many extra expenses incidental to packing, shipment, &c. exhausted the available funds. The Royal Society of London, however, deeming it of great importance that the telescope should be fitted for photographic work, kindly took it upon themselves to order the requisite appliances, which, I believe, are now in course of construction by Mr. Grubb, the maker of the telescope. We are not yet informed of the exact cost of the apparatus, but it will be necessary to relieve the Royal Society of its self-imposed burden." I have had no intelligence of this apparatus beyond that conveyed by Mr. Grubb in letters to Mr. Le Sueur. I believe it is in course of construction, but I have no definite knowledge of the fact, or of its probable cost when completed.

The telescope has now been fairly at work for seven months, but unfortunately the frequent cloudy and hazy weather which interfered so considerably with the transit and zone observations very much limited the number of nights upon which good work could be done.

At first the large mirror (called A) was mounted in the telescope, but the definition with either of the small mirrors was not good, and it soon got strongly tarnished; subsequently, in September, mirror B was substituted, and the telescope performed much more satisfactorily. With respect to this and other matters, Mr. Le Sueur reports to me as follows:—

"The general work with the Reflector has consisted in the exa-

mination of the nebulæ. For obvious reasons those already figured have been specially attended to, and fair progress has been made both in their delineation and spectroscopic examination.

"As might be expected, the nebulæ, as seen with the Melbourne Reflector, in many cases differ considerably from the Cape of Good Hope drawings; but, as far as may be judged at present, the differences do not appear to be greater than might be expected from the difference of the instruments. An exception, however, must be made in favour of the nebula in Argo, whose present appearance is not readily accounted for, except on the supposition that enormous changes have taken place since the date of Sir John Herschel's drawing. Spectroscopic observations of the star η in this nebula, and of Jupiter, have afforded interesting results.

"Of work out of the usual course, Neptune was examined on many occasions when near opposition, but the closest attention failed to detect a new satellite or to afford any suspicion of peculiarity in the figure; the position of the planet, however, was unfavourable, and the atmospheric conditions on no occasion unexceptionable.

"Winnecke's periodic comet was picked up early in August, but was not observed for position before October; a set of observations were then made extending to October 25th, unfavourable weather preventing the following up of the object to a later date; from accounts, however, which have reached us, it appears that the Melbourne telescope was, by a few days, the last in the field.

"Large mirror A was taken down and replaced by B in September; the definition of A in combination with either *a* or *b* was not satisfactory; the reason for replacing it by the other, however, was on account of its tarnished condition.

"The performance of the combination B*a*, as regards definition, was considered much better than that of A*a* or A*b*; *a*, however, being considerably tarnished, was taken down and replaced by *b*; but it was found that to use this combination with advantage a more extended motion of the small mirror arm was required; *a*, which in the meantime had been polished, was therefore replaced, and has been at work satisfactorily for the last five months; *b* remains in *statu quo*. A slight mechanical alteration of the focusing arrangements will make it possible to use this mirror without refiguring; its definition is equal to *a*'s."

In reference to the Melbourne telescope, the President of the Royal Society of London, Sir Edward Sabine, in his annual address, delivered on the 30th of November, 1869, says, "Its performance since erection does not appear to have given altogether the same satisfaction at Melbourne that it did at Dublin; but the defects complained of may arise partly from an imperfect knowledge of the principles of the instrument and inexperience in the use of so large a telescope, partly from experimental alterations made at Melbourne, and partly from atmospherical circumstances. Those who are acquainted with the difficulties which Sir J. F. W. Herschel experienced at the Cape, will not be surprised that they should be felt at Melbourne to a much greater extent, on account of the far greater size

of the speculum. But I have no doubt that, if the instrument be kept in its original condition and as carefully adjusted as it was in Dublin, it will perform as well in ordinary observing weather." Now this would convey a wrong impression; and I do not think the defective performance alluded to can in any way be accounted for by this supposition—the principles of the Cassegrain telescope are fully understood; no experimental alterations have been made except such as were actually required to get the telescope to perform at all; and from all I can ascertain the adjustments have been far more carefully and critically made than they were in Dublin. No simpler explanation of the defect can be made than that mirror A is not good.

On this point Mr. Le Sueur remarks:—"With reference to the paragraph in Sir Edward Sabine's address to which you have called my attention, I beg to state that the only alteration in the optical arrangements concerning the *necessity* of which there is any question, is the insertion of an opaque annular disk at small mirror.

"This disk at first met with much opposition on the part of Dr. Robinson and Mr. Grubb, but the former (who has had more experience with Cassegrains than any one) now agrees that the disk is necessary if the eyestops are retained in the places originally assigned to them by Mr. Grubb.

"The disk can have no injurious effect on the definition, its sole disadvantage is the interception of a small percentage of light; but this is in some degree counterbalanced by an increase of the field of view consequent on having the stops in a position nearer to the eye-lens than that which may be considered the normal one.

"Possibly, for certain purposes, the increase of field is not of so much consequence as the slight diminution of light; but, at any rate, the disk solves in a ready and effective manner the difficulties introduced by the makers themselves, who evidently intended that the stops should retain the positions which they now have. I allude, of course, merely to the distance of the stop from eye-lens; the lateral adjustment is so simple that any error therein of sufficient magnitude to produce any disturbing effect is entirely out of the question."

The mirror A on the polishing machine will have to be repolished before it can again be used. There seems to be some peculiar condition of the metal which appears to render it more liable to tarnish than the other. A kind of reticulation of the surface is apparent; and, on microscopic inspection, masses of good coloured speculum metal seem to be interlaced by a redder alloy that oxidizes more readily than the white part—a kind of segregation has evidently taken place.

Nearly everything is prepared for the operation of polishing. A test-object is erected in the park at a convenient distance from the machine, and a platform for testing the figure during the operation has been provided, and most of the minor arrangements for preparing the polisher are complete. No proper rouge is obtainable here, and a quantity was ordered several months ago from London. We shall probably have to eventually make it on the spot. The great

delicacy and difficulty of this operation is fully appreciated. Mr. Le Sueur had the advantage of watching the process of grinding and polishing in Dublin, and has since had some practical experience; and as the same apparatus with which the mirrors were first prepared will be used, there is no good reason why, with proper care, the repolishing should not be performed successfully. In the case of the small mirror *a*, the polishing was satisfactorily accomplished, a good reflecting surface was recovered, and the figure (which is a very critical affair) is *certainly as good* as at first.

It is a somewhat difficult matter to speak critically of the merits of this telescope, as our only standards of comparison are other large reflecting telescopes, of which there are only three in the world that reach the dimensions of this, namely, Herschel's, Lord Rosse's, and Mr. Lassell's, and these are again of a different form, which renders even a comparison difficult. I have had no experience with the reflectors in question. Mr. Le Sueur, however, observed with Lord Rosse's for some time, and his impression is that, even allowing for the difference of aperture (Lord Rosse's being 6 feet and ours 4 feet), the definition of ours with mirror A was not so good. So far as I can speak from practical acquaintance with this reflector, I was not satisfied with the performance of mirror A in either combination of small mirrors; on the planets, as well as on stars, the definition was very unsatisfactory.

But mirror B performs very much better, and the telescope will now doubtless compare favourably with others of its class and dimensions. Mr. Le Sueur entirely coincides with me in this expression of opinion.

The experience which can only be obtained by years observing with large reflectors is the only safe guide in forming an opinion of the comparative merits of a telescope of this kind, and that it is an experience which no one outside of Great Britain possesses; it is more than probable, therefore, that the want of it has led ourselves and others to expect more from optical science, even in its present advanced state, than we had any right to, and that we looked for as comparatively a perfect telescope as is sometimes obtained with smaller apertures.

What has been done, however, unmistakably shows that it is capable of grasping the work for which it was originally intended. Though perhaps not reaching that perfection for feature-work of planets which was hoped for, we have no reason to doubt that it will be fully up to our anticipations with respect to celestial photography.

As far as the mounting and mechanical arrangements are concerned, when we consider the immense mass of the moving parts, and the methods hitherto adopted for large reflectors, we have every reason to be satisfied.

* * * *

ROBT. J. ELLERY.

March 31, 1870.

Mr. Grubb's Remarks on the Specula of the Great Melbourne Telescope, and the Melbourne Reports connected therewith.*

The metallic specula supplied with G. M. T. consist of two large mirrors (denominated A and B), and two small (*a* and *b*). Mirrors A and B were cast from the same admixture of metals, previously prepared. Speculum A is that which was generally used here during the temporary erection of the equatorial mounting, and which was fully tested and approved of by the Telescope Committee of the Royal Society. Speculum B was not considered as finished at the time of the Committee meeting here for the trials. It was subsequently polished, and finally approved of by Mr. Le Sueur, my son, and myself, as fully equal to speculum A. The specula were, in accordance with the instructions of the Committee, previous to packing, coated with a solution of shell-lac in alcohol, as practised by Mr. Lassell.

Having thus premised, I turn to the Melbourne Report for the last year, which, at page 12, states as follows.—

"At first the large mirror (called A) was mounted in the telescope, but the definition with either of the small mirrors was not good, and it soon got strongly tarnished. Subsequently, in September, mirror B was substituted, and the telescope performed much more satisfactorily."

All which is readily accounted for by statements in letters previously received here from Mr. Le Sueur, viz.:—

1st. That he found it impracticable to entirely remove the shell-lac coating.

2nd. That with his best exertions, the surface remained sticky in parts.

3rd. (On being questioned as to the particulars of his process, and the possibility of his having used methylated spirits instead of alcohol), he admits having used methylated spirits, and states his process to have consisted in brushing it over the surface, and then mopping well with water. (He excuses the using of methylated spirits as having been overruled by Mr. Ellery.) In his letter to Professor Ellery, published with the last Melbourne report, the process is again described, but with no essential difference. It is verbatim as follows:—

"The varnish was removed from mirror A in the following manner:—Large pieces of cloth were placed on the surface and saturated with methylated spirits; when the varnish was sufficiently softened, as much as possible was removed by mopping up with clean cloths (this process was repeated many times), distilled water was then freely used, and finally pure alcohol."

The Melbourne Board, referring to the process mentioned in the above extract from Mr. Le Sueur's letter, states that "It is difficult to see how it can have had any deleterious effect upon the polish of the speculum."

No such difficulty, however, exists on considering the rationale

* Excerpt from a pamphlet by Thos. Grubb, F.R.S., March 23, 1870.

of the treatment, which includes the deposition on the polished surface of a substance differing essentially from the original, and only soluble in *very* strong alcohol. I cannot understand how Mr. Le Sueur came to the conclusion stated in his letter, in the appendix to the report, that the method he employed could not be supposed to have any deleterious effect on the polish, after stating, as he had previously done, that he found it impracticable to entirely remove the shell-lac coating, and that with his best exertions the surface remained "sticky" in parts, and afterwards the speculum tarnished quickly.

The process was radically bad, and it is evident by his own statement, that the surface was, after his attempts at cleansing it, *left in an unclean state*; and to expect under such circumstances that it would not tarnish, and that quickly, would be quite absurd*.

I shall here add a few corroborative facts, viz.:—After months of use at Dublin, this speculum's surface exhibited no tarnish. 2nd. After the speculum arrived at Melbourne (some portion of the surface being denuded of varnish, probably with alcohol), it was described in the report for that year as exhibiting a "very fine polish." 3rd. Speculum B, cast of the same metal and coated with the same varnish as A, has, according to Melbourne reports, been *really* cleaned from the varnish, and has not since shown sensible tarnish, although the soap used in cleaning it was likely to have dimmed it. 4th. The centre-piece, of eight inches diameter, cut out of speculum A forming its central hole, was polished here shortly after speculum A had been, and on several occasions it has been coated with the lac varnish, which, after remaining for months, has been removed without difficulty, simply by alcohol (but no water), and without injury to the surface, which still remains in good working order. There is abundant evidence, especially in the last item of the foregoing, to show that speculum A has be-

* A somewhat different statement appears from Mr. Le Sueur in his letters in the appendix to the report. He says that after his "cleaning process," the mirror A was then "fairly bright, and appeared perfectly clean; close examination afterwards showed that a faint trace of lac still remained on the surface, which became considerably tarnished after short use."

He attributes the tarnishing which ensued not to the thin film, but to the metal (although mirror B of same metal has not sensibly tarnished), and he attributes the streaky marks, which he could not remove, not to his own imperfect removal of the film, but (contrary to all evidence) to the brush used in laying on the varnish here.

Respecting mirror B, his letter aforesaid states, "That the varnish was removed from B by means of naphtha, mopping as before, washing with soap and water, and finally with distilled water. The mirror, though at no time very bright, is still in fair working order." From the last statement (this mirror having left this with an excellent polish), I conclude that it also has suffered, though compared with mirror A but little, from the *water process*. Water, though probably innocuous in itself, appears to have the property of throwing down other substances upon the polished surface. For instance, if on stopping the polishing machine, the wet polisher be allowed to remain stationary for a short time on the speculum, a distinct impression of the pitchy patches of the polisher may be observed. Mr. Le Sueur had opportunities of witnessing this while here. Why he used water at all, more especially in the case of speculum B, I cannot explain.

come unfitted for present use, *solely from injudicious treatment at Melbourne.*

Referring again to the paragraph of the report which states that—

"At first, the large mirror A was mounted in the telescope, but the definition with either of the small mirrors was not good, and it soon got strongly tarnished."

Now, with respect to mirror B.

This mirror B appears from the report to have been for some time in use with the small mirror *a*.

The only fault found, apparently, with the telescope so constituted is, that it gives a slightly cruciform image of a star—a fault which did not appear when mirror B was finally tested and approved of here, as being fully equal in definition to mirror A. The fault is not, therefore, fairly attributable to the large speculum B, but probably to the small mirror *a*, which was repolished at Melbourne, as nothing short of the most scrupulous care in the supporting of a mirror on the polishing machine could insure freedom from strain during the polishing, and a perfect figure when removed.

The usual and palpable methods for ascertaining where the defect lies (such as turning the small mirror round in its cell, or substituting the other—unrepolished—small mirror) do not appear to have been tried. Neither was examination made of whether any of the balls of the supporting levers of the large speculum had got out of place during the transit (a thing quite possible, and which actually occurred in the case of another speculum lately constructed here).

Whatever be the cause of this appearance, it must (judging from the accounts of the astronomers) be very slight. Mr. Le Sueur, writing, says, "It is not a conspicuous feature by any means, nor is it permanent," and that it most probably arose from some abnormal condition of the levers, and that once remedied, they would get rid of this cruciform image; while Professor Ellery (the Government astronomer), in his report, states that the mirror B performs very much better than A; that the telescope will now, doubtless, compare favourably with others of its class and dimensions, and that Mr. Le Sueur entirely coincides with him in this expression of opinion.

XXIV. *Sir Edward Sabine to Mr. Verdon.*

13 Ashley Place, May 16, 1870.

My dear Sir,

The accompanying correspondence originated in a letter (No. 1) addressed by Mr. H. A. Severn, of the Union Bank of Australia at Melbourne, to Mr. Charles Watkins Merrifield, a Fellow of the Royal Society of London, by whom it was officially transmitted to myself as President of the Society, in a letter which is marked No. 2. Copies of these letters were, without loss of time, forwarded to the

Members of the Committee, by whom the onerous and responsible office of superintending the construction of the Great Reflector for the Melbourne Observatory had been undertaken; and in addition to these gentlemen, at Mr. De La Rue's suggestion, to Sir John Herschel, who had taken an active part in the correspondence which preceded the undertaking. No. 3 is Mr. De La Rue's reply, No. 4 Sir John Herschel's, and No. 5 is Dr. Robinson's.

It has appeared to me the most fitting proceeding that you should be furnished with copies of these letters, with a view to their being known to Mr. Ellery, the Superintendent of the Melbourne Observatory; and I beg, therefore, to transmit these copies to you, with a request that they may be brought to Mr. Ellery's notice.

I remain, my dear Sir,
Very sincerely yours,
EDWARD SABINE, P.R.S.

P.S. Copies have also been sent to Mr. Merrifield.

G. F. Verdon, Esq., C.B.

ENCLOSURES.

Mr. Merrifield to General Sir Edward Sabine. (No. 2.)

Institution of Naval Architects,
9 Adelphi Terrace, London, W.C.,
April 22, 1870.

DEAR SIR EDWARD,

I have received the letter, of which the enclosed is a copy, from an old friend of mine, whom I know to be a clever and shrewd practical man, and not without experience of telescope work. He has made his own instruments as well as used them. Beyond this I have no knowledge as to how far his acquaintance with the subject extends. It is one of which I have myself no experience whatever.

I think, however, I shall be doing what is right in sending on his letter to you, as it contains some statements to which you will know, far better than I do, what weight to attach, and which therefore I ought not to keep from you.

I repeat, however, that I am not myself capable of forming any opinion whatever on the subject.

Believe me, dear Sir Edward,
Yours very truly,
CHARLES W. MERRIFIELD.

To General Sir E. Sabine, P.R.S.

Mr. H. A. Severn to Mr. C. W. Merrifield, F.R.S. (No. 1.)

Union Bank of Australia,
Melbourne, Feb. 27, 1870.

(Copy.)

MY DEAR MERRIFIELD,

As I have been rather active (for a long time) in a fight against

the Great Melbourne Telescope, and as it is very likely that the subject will be discussed by your Royal Society, it may be right that you should know why I maintain it *will and must prove a failure*!

The first requirement in order that a reflector may work well is equiformity of temperature, in order that the *molecules* of the metal of the speculum may be at rest. Now, as we very often have the temperature 136° all day, and perhaps in the evening it at once (in two hours say) falls to 60°, and these changes, with more or less vigour, go on all the year round; how, I ask, can we expect a 4-feet metal under such circumstances to retain its form? Were it a front view or Newtonian it certainly would stand a better chance; but how can we expect the 4-feet metal to retain even a decent focus here, more particularly when its errors are of course aggravated by the small convex speculum. What do you suppose is the length of the focus or aberration in one metal? The least is 5 *feet* 6 *inches*, the other more. Mr. Le Sueur told me this himself; and yet, in the face of this tremendous distortion, Ellery, who is unpleasantly placed (the Government Astronomer) here, pretends that it is a success, and is indeed the cause of your General Sabine running away with conclusions drawn from wrong inferences. Truly η Argo is very much smaller; and we in the south are all well aware of the fact; but of the displacement of the star η (of course by the translation of the solar system) with respect to the nebulæ around, we are by no means so certain. The big instrument so disfigured Orion, that I am strongly of opinion that the conclusions arrived at cannot be confirmed. The instrument has cost us colonists £14,000; and whatever *it was in Ireland*, and however well it defined there, it is, beyond all doubt, very far from perfect in this country.

The difficulty of collimation is all but insuperable, and when perfect at one elevation gets out at any other. Fancy how jolly! I suppose for the same money we could have got a 20-inch refractor!

These remarks refer to a subject of much importance to us—to me in particular, as I predicted the effect of temperature long before is was set up; and though I am contradicted, those who know are gradually giving way. As I have profound respect for you and for any opinions or remarks you might make in this matter, I have on that account stated only facts. I commit them to you as facts; you can therefore make any use you please of them.

It is a grievous failure; it will certainly go far to prove the error of constructing a large Cassegrain, or any kind of reflector, for this country.

. . . . . .

(Signed) H. A. Severn.

To C. W. Merrifield, Esq., F.R.S.

Mr. De La Rue to Sir Edward Sabine. (No. 3.)

The Observatory,
Cranford, Middlesex, W.,
April 26, 1870.

DEAR SIR EDWARD,

I return you Mr. Merrifield's and the copy of Mr. A. Severn's letters.

In the first place, it is quite true that great and rapid changes of temperature do interfere with the performance of any telescope, and more with that of a reflector than with that of a refractor.

Change of temperature interferes chiefly by causing currents of air within the tube and about the metals, and not nearly so much by any change of form of the mirror. No doubt that if any *one part* of the mirror were heated, then this local heating would distort the figure; but I do not believe that the changes of temperature which occur in a day would materially affect the figure of the mirror, because they could only very gradually and pretty uniformly increase the temperature of the metal.

Rapid and great changes of temperature would affect nearly to the same extent the performance of Cassegrainian, Front-view, or Newtonian, because the chief disturbance is from currents of differently heated air passing before the mirrors and along the tube.

My opinion of the value of the great Melbourne telescope is in no way shaken by Mr. H. A. Severn's remarks. By diligently working the great Melbourne telescope, much may be done to advance astronomical knowledge. Of course some days in the year will not be so good as others, and the bad days may be more frequent than in England; nevertheless good work can be done with it. I think you may send Mr. Severn's letter to Dr. Robinson; there is nothing contained in it of which he cannot easily dispose*. I suppose the cost of the great Melbourne telescope, placed at £14,000 by Mr. Severn, is erroneous.

Sir John Herschel could give us the benefit of his experience at the Cape with an 18-in. I would recommend the letter being sent on to him, and you are at liberty to send this also.

I am,
My dear Sir Edward,
Yours very sincerely,
WARREN DE LA RUE.

Sir J. Herschel to Sir Edward Sabine. (No. 5.)

Collingwood, April 28, 1870.

MY DEAR SIR EDWARD,

Any change of temperature, if rapid enough to affect the surface of a speculum to a sensible degree before reaching the interior by conduction, must of course bring the metal into a state of strain. The ques-

* Dr. Robinson was at the time unwell.

tion is how this will affect its optical action. Much will depend on how the speculum is supported. If it rest on a bed of badly conducting material, as was the case with mine, the polished surface will be expanded or contracted sooner than the back, and (taking the case of contraction by cold at night after a hot day) the polished surface will contract in area quicker than the back, the mirror will grow more concave, and the focus shorter. Still, since the whole surface is exposed equally, and the total contraction is very small, I should expect that a *change of focal length* would be the main optical effect, and that aberration (arising from deviation from parabolicity), though probable to some small amount, would be but an effect of the second order—a small fraction of a small quantity. We had hot days and cool nights at the Cape, though not to such violent extremes, but I do not recollect ever having reason to complain of this form of annoyance.

Next, suppose the mirror supported at the back on metallic points or bearings of very small area, leaving perfectly free access of cold air to the back, and free circulation. In this case back and face would contract equally, and the effect would be that no general flexure of the metal, one way or other, would take place, and the focal length would therefore so far as this (much the most important) cause of distortion, remain unchanged. A certain (probably slight) amount of aberration might arise from pressure on the internal molecules.

What Mr. Severn says respecting the actual amount of aberration is unintelligible. "What" (he says) "do you suppose is the length of the focus or aberration in one metal? The least is 5 feet 6 inches, the other more" (sic in Mr. Merrifield's copy). If for "*length*" should be read *lengthening*, and for "*one metal*" be read *in the case of one metal*, it conveys, no doubt, a distinct meaning. But a longitudinal aberration of 5 feet 6 inches, produced by such a cause as change of temperature of 70° in a speculum otherwise perfect, of only 32 feet focal length, appears to me incredible. It would require the extreme ray to be deviated by 31′ from its proper direction; to effect which the extreme annulus of polished surface would require to become inclined *backwards* from its proper inclination (tangential to the paraboloid) 15′ 30″; to produce which change of inclination the extreme rim of the metal would require to be sprung backwards by $\frac{11}{1000}$ of an inch—a quantity quite preposterous—enough, I should think, to break the metal. Moreover, such an effect would require *that the back should contract more* than the face.

By far the worst effect (and, so far as my experience goes, the only formidable one) of rapid changes of temperature arises from mixture of hot and cold air in motion, which the rays reflected from the speculum have to traverse before reaching the eye. This is, however, at least to a great extent, counteracted by the latticed structure of the tube. Still there is (if I apprehend the figure given in the "Correspondence" &c. rightly) a portion near the mirror not latticed. Would it weaken the structure too much to pierce this with a few large or a great many small holes? Also, would it not be advisable,

just before sunrise, to muffle the large mirror up with thick blanketing, and stuff the hollow in front with woollen mattresses, to keep off that terrible temperature of 130°? in the day-time?

After all, *there is* the telescope, and *there are* the observers; and it is by its work in its best action, and not in its worst, that its astronomical efficiency will come to be estimated. Mr. Severn's *à priori* condemnation, and his dictum "that it will and must be a failure," may stand for whatever may be supposed to be its worth; but I think Mr. Ellery and Mr. Le Sueur will do well not to be discouraged by any such denunciation, but keep steadily working on; and, above all things (as an immediate object), send us home some careful and dependable drawings, *both* of the nebula in Argo *and* that of Orion (which Mr. Severn says it *disfigures*), as seen in it under as favourable circumstances as the climate on the whole affords, and leave us to judge of the amount and nature of the disfigurement.

I remain,
My dear Sir Edward,
Yours most truly,
J. F. W. Herschel.

Rev. Dr. Robinson to Sir E. Sabine. (No. 5.)

Observatory, Armagh,
May 7, 1870.

My dear Sir Edward Sabine,

It is a curious coincidence that so soon after my letter to you on the matter of the great Melbourne telescope, in which I referred to the misrepresentations about it, such a notable specimen of them should have reached you!

Mr. Severn, in his letter to Mr. Merrifield, shows so great an amount of prejudice, and his animadversions have been so well answered by Sir John Herschel and Mr. Warren De La Rue that I should not have thought it necessary to notice them, but for some points, showing singular recklessness in assertion, which neither of those gentlemen have touched on.

The first of these is his statement that at Melbourne "the temperature often reaches 136°, and falls in two hours to 60°;" and that "these changes, with more or less vigour, go on all the year round."

Now, it is incredible that the temperature in the telescope-house can rise to anything like this during any part of the day, especially if the roof be (as I suppose) painted white. The only information I have as to the temperature of Melbourne is derived from "The Discussion of the Meterological Observations at Melbourne during the years from 1858 to 1863." I find there that the maximum observed during those five years was 111° on the 14th January 1862; and that the mean maximum for that month during the five years was 76°·8, and the minimum 59°—giving an average diurnal range of

17°·8. As the maximum occurs between 1 P.M. and 2 P.M., the temperature must evidently be much less towards sunset.

If Mr. Severn has not spoken entirely at random, it may be conjectured that he has given the temperature of a thermometer exposed to direct solar radiation.

It is quite unnecessary to make any remarks on his language respecting Mr. Ellery, or on his statement that "η Argus is not displaced (of course by the translation of the solar system) with respect to the nebula."

His assertion that "the big instrument so disfigures Orion that he thinks the conclusion arrived at cannot be confirmed," amounts only to this, that it shows that nebula in a very different way from what Mr. Severn's telescopes do.

That the instrument has cost the colonists £14,000 will probably surprise you as much as it does me.

His statement, that "the difficulty of collimation is all but insuperable," and that "when perfect at one elevation it gets out at any other," if it have any foundation, must have arisen from this, that the bolts which attach the speculum-box to the tube, those which connect the two portions of the tube, those which attach the tube to the declination axis, or those which fix the arm of the small mirror, were not screwed home at the time he examined it. Nothing of the sort was observed in Dublin. The levers which support the speculum are so strong that any tilting of it at different altitudes is impossible. The flexure of the tube tends to diminish any change of collimation, which can only be produced by a lateral shifting of the mirror in its box. This cannot exceed $\frac{1}{10}$ of an inch; and it is easily computed that this amount of shifting can only produce at its *maximum* an error of 42 seconds.

As to his statement that "it is is a grievous failure, and will go far to prove the error of constructing a large reflector for this country," it is probable that it will have much less weight with the public than the good work which the telescope has already done, and is doing.

I wrote to Mr. Grubb about Sir J. Herschel's suggestion about ventilating the lower part of the tube, and enclose his answer.

Yours ever,

T. R. Robinson.

Sir Edward Sabine, K.C.B.

XXV. *Sir Edward Sabine to Mr. Verdon.*

[Extracts.] 13, Ashley Place, June 20, 1870.

My dear Sir,

I return Mr. Ellery's letter of January 3rd*, with thanks for its

* This letter refers to a private note addressed by Mr. Ellery to Mr. Vernon, in which he describes the departmental difficulties which had arisen from Mr. Le Sueur's entering on his duties as Observer.

perusal. It throws additional light on the mischief which has arisen for want of Mr. Le Sueur being led on his first arrival at Melbourne (several weeks I believe before the arrival of the telescope), to comprehend his true position both in the Observatory and in the Colony, his duties and responsibilities, and where he was to look for aid in all ways, and particularly in obtaining whatever might be found requisite for the successful establishment and working of so novel an instrument. . . . Until the letter from the Melbourne Government arrived a few weeks ago, we had no other information than could be gleaned from letters received by Mr. Grubb (the maker of the telescope), from the Colony, or from the private letters of Mr. Le Sueur to Professor Stokes, by whom Mr. Le Sueur had originally been recommended for his present appointment. From these last-named letters, supplying as they did the only information which the Committee were enabled to give to the public on a matter on which great public expectation existed, selections were made and printed in the Proceedings of the Royal Society.

I remain, my dear Sir,
Faithfully yours,
EDWARD SABINE, P.R.S.

XXVI. *Sir Edward Sabine to Professor Stokes, Sec. R.S.*

The Royal Society, Burlington House,
January 10, 1871.

DEAR STOKES,

Mr. Verdon, whom I happened to meet some days since, told me that he fully believed that Mr. Le Sueur had finally decided on resigning his post at the Melbourne Observatory. As yet I am not aware that any notification to that effect has been addressed by him either to the President of the Royal Society, or to the Chairman of the Telescope Committee, from whom he accepted the appointment, carrying with it instruction at the Cambridge Observatory, at Mr. De La Rue's Observatory, at Lord Rosse's Observatory, and at Mr. Grubb's Manufactory, all given in the view of his future usefulness at Melbourne. It is quite possible that such a communication may be on its way from Melbourne.

(Signed) EDWARD SABINE, P.R.S.

XXVII. *Sir Edward Sabine to Professor Stokes, Sec. R.S.*

The Royal Society, Burlington House,
February 9, 1871.

DEAR PROFESSOR STOKES,

I have recently been informed officially by the Agent-General for Victoria that Mr. Le Sueur has resigned the appointment which he had accepted in the Melbourne Observatory, as Observer with the Great Telescope, recently completed for that Observatory under the superintendence of a Committee of the Royal Society. Mr. Verdon made no statement of the causes which may have led to Mr. Le Sueur's resignation of an appointment for which he had been recom-

mended by yourself, and had received in consequence of that recommendation much valuable instruction at the Observatories of Cambridge, Cranford, and Parsonstown. As the continuation and completion of the "Correspondence" regarding the Melbourne Telescope, printing by the Royal Society, is now in hand, and as it may be deemed desirable by the Committee of the Royal Society that some statement from Mr. Le Sueur himself should appear in that publication, and as I have learnt indirectly (from yourself) that Mr. Le Sueur is now in England, I would venture to suggest to him through you (to whom his appointment was due), the expediency of his furnishing the Committee with a statement, if he thinks proper to make one, of the reasons which caused his resignation, such statement being one that may suitably be included in the completion of the publication referred to. It may be addressed either to the Committee or to myself, as Mr. Le Sueur may prefer. If to me it will pass at once into the hands of the Committee.

(Signed) EDWARD SABINE, P.R.S.

XXVIII. *Mr. Le Sueur to Professor Stokes.* (Copy received by Sir Edward Sabine subsequently to February 9th, 1871.)

Melbourne Observatory,
May 21, 1870.

DEAR SIR,

I am sorry to inform you that, after mature consideration, I have decided to resign my office in connexion with the Melbourne Telescope.

The trammelled position in which I am placed is so far different from that which in taking office I had every reason to expect, and above all is so damaging to the work, that I can no longer accept its conditions.

Please convey to the Members of the Committee my deep regret at feeling myself unable any further to carry out the mission entrusted to me: if, however, I had thought that the work would suffer by my abandoning it, I would have endeavoured to school myself into a resigned acceptance of conditions, however irksome and unsatisfactory.

Nearly all the nebulæ figured in the C.G.H. have been reviewed, and the information gathered will serve as a good groundwork for future reference.

I have taken the precaution of fully instructing, as far as lay in my power, the assistant workmen and others here in the manipulatory details of polishing. Since the wet weather set in I have been constantly engaged in frequent repolishings of small mirrors which I have procured here; I hope, moreover, to repolish large mirror A; the experience gained on the small ones makes me feel more confidence as to the result than I at first anticipated.

Yours very truly,
A. LE SUEUR.

To Professor Stokes, Sec. R.S.

Abstract of Dr. Robinson's Description of the Great Melbourne Telescope.

This telescope has been minutely described, and the details of its construction explained at considerable length, in a paper published in the Philosophical Transactions, 1869, by the Rev. T. R. Robinson, D.D., F.R.S., the Chairman of the Telescope Committee, and T. Grubb, Esq., F.R.S. An abstract of it, containing some of the most interesting particulars, is given here to complete the continuity of the correspondence.

Of the introductory part it seems only necessary to give the motives which induced the Telescope Committee to select a reflecting telescope instead of an achromatic, and the Cassegrain instead of the Newtonian form of the reflector.

Dr. Robinson has shown that the intensity of light after two reflections from speculum metal, allowing for the light intercepted by the small mirror, is 0·401; the coefficient for the intensity of light transmitted by an achromatic is not so simple, for it varies with the aperture of the object-glass. Its expression is exponential, which is given; the constants depending on reflection at its four surfaces being computed from Fresnel's formula, and the constants of absorption being determined by a series of photometric experiments on object-glasses and specimens of optical glass. It follows from this that an achromatic equiluminous with a 4-feet reflector must be 34·4 inches. At present there is no probability that such an object-glass could be constructed; if practicable, its cost would be enormous, and the difficulty of mounting and using it very great. The selection of the Cassegrain construction was not made without considerable discussion; it has been little used, probably in consequence of Newton's objections to it, the principal of which was that less light is reflected by its small mirror at an incidence nearly perpendicular than in the Newtonian, where the incidence is 45°. This is true for reflecting surfaces like glass, but not for metals. In these, as has been shown both by theory and experiment, the reflection decreases from the perpendicular incidence to one depending on the incidence of maximum polarization, and then increases till at 90° the reflection is total. M. Jamin has shown that for speculum metal the intensity of red light is at a perpendicular incidence 0·692, but for 45° only 0·646. For the other rays the intensities are less, but follow the same law, so that the advantage is actually in favour of the Cassegrain; its principal defect is the magnitude of the second image, which is magnified by the small speculum from 5 to 6 times. It therefore is unfit for photographic purposes, and requires eyepieces of great focal lengths to give the lower magnifying-powers; but this is far more than counterbalanced by the observer being always near the ground, and requiring to shift his position but little for considerable movements of the telescope; while in the Newtonian, of equal power, the observer must be supported by an apparatus moveable through a large space in azimuth; and near the zenith, requiring an elevation of little less than 40 feet, the employment of which is laborious, and may be dangerous.

For a Cassegrain on such a scale Dr. Robinson did not think it safe

to trust to the formulæ usually given in treatises on optics, and determined its constants on the following assumptions. In the Cassegrain it is necessary that an eyestop should be placed behind the eyepiece (supposed to be Huyghenian) at a distance from its second lens, a little more than half the focal length of that lens, and of a diameter equal to the image of the large speculum formed by the small one and the eyepiece.

(1) The eye placed close to this views the last image at the distance V = the distance of minute vision. Knowing V, and the distance of the eyestop from the lens, it is easy to deduce the positions of the 4th, 3rd, and 2nd images, with respect to the field-lens of the eyepiece.

(2) The opening of the large speculum should be as large as the aperture of the small one.

(3) The aperture of the field-lens of the lowest power should be of the same size, that the largest possible field of view may be obtained.

(4) The lowest power must be such that the eye may take in the whole of an emergent pencil.

(5) The aperture of the small mirror must be able to receive all the light reflected by the large one.

From these considerations, a quadratic equation is found giving in terms of known quantities the focal length of the field-lens of the lowest power; and from this the other constants of the telescope are determined, except the focal length and aperture of the great speculum, which are selected by the optician.

Of these quantities, M, the lowest magnifying-power, is found by dividing the aperture of the great speculum by the breadth of the emergent central pencil at the eyestop. According to Sir W. Herschel, this breadth cannot exceed 0·2*, and therefore M = 240.

n, the ratio of the aperture to the focal length of the lenses, is generally made by opticians 0·5.

There is some doubt about V; but the value (8 inches) adopted by Sir W. Herschel seems entitled to most confidence.

r is nearly equal to the diameter of the emergent pencil.

The constants computed for the telescope were—

Large speculum, focal	$F = 366$	Distances of 1st and 2nd images... ..	$d = 61$
Large speculum, aperture	$A = 48$		
Small speculum, focal	$f = 74{\cdot}71$	From the second speculum	$d' = 332{\cdot}31$
Small speculum aperture	$a = 8{\cdot}05$		$\frac{d'}{d} = 5{\cdot}4477$

Lowest power, focal $f = 16{\cdot}10$, M = 240, field of view $\theta = 14'{\cdot}32$, equivalent focus = 1994†.

* In such a telescope as the Great Melbourne Telescope this can only be true for faint objects, as a star of the first or second magnitude is so brilliant that the pupil contracts to a diameter considerably less than this; but the telescope was primarily intended for nebulæ.

† These equations relate to a central pencil; for in the case of a pencil near the extremity of the field of view, some of the rays, about a ninth of the whole, would be lost. But this may be obviated by making a somewhat larger than the computed value, in the Great Melbourne Telescope about a seventh.

As soon as these were decided on, the contract (which, beside the telescope and duplicate large speculum, included an apparatus for polishing and a steam-engine for working it) was signed at the close of February 1866, and the work was urged rapidly forward. Of this, the most important, and at the same time the most difficult part, was the formation of the large specula. The alloy of which they were cast was Lord Rosse's, composed of four equivalents of copper to one of tin; it possesses more power of resisting tarnish than those which deviate a little on either side of this proportion, and is probably as reflective as any. The liability of good speculum metal to tarnish is much exaggerated when due precautions are not taken.

(1) The operations of casting and annealing were conducted essentially as described by Lord Rosse, with such modifications as the case required, or as were indicated by experience. The plan for lateral support of the speculum proposed by Mr. Grubb requires that the speculum should be cast with a central band about $\frac{1}{4}$ inch thick, and $\frac{1}{3}$ of the speculum's thickness in breadth. The edge of the mould was therefore formed not of sand, but of a strong sheet-iron hoop, 8 inches deep, and of the same diameter as the bed of hoops, which, in imitation of Lord Rosse, formed the bottom of the mould. It was strengthened internally with rings of angle-iron, and its lower edge turned true, and it was lined with loam moulded to the required shape. When this was fully dried, it was attached by clamps to the bed of hoops, and the joint luted with a little wet loam.

(2) A peculiar arrangement was applied to support the mould. It was at first attempted to cast the speculum with its central opening, by attaching to the centre of the bed of hoops a core 8 inches in diameter; there was reason to suspect that the waves of melted metal which were formed as it passed this core did not unite perfectly; at least, in the direction where they met, there were seen faint surface-markings (called by Lord Rosse "crow's feet"), which indicated such imperfect union.

To provide for a more regular diffusion of the melted metal over the mould, the cast-iron frame supporting it was made to turn at the end next the crucible, parallel to that of the cradle, which shall be presently noticed. The other end was raised or lowered by a wedge driven by a rack and pinion under the floor, which again was actuated by a vertical shaft at a convenient distance from the casting. As the central core interfered with the free flow of the metal, it was raised $1\frac{1}{2}$ inch above the surface of the mould, and properly supported there.

Fig. 1 is a plan of the casting- and annealing-room, 36 feet long by $16\frac{1}{2}$ wide. A, the hot hearth for heating the bed of hoops, which (as described by Lord Rosse) forms the bottom of the mould; it consists of four low pillars of brick, with intermediate grates for coke fires.

B is the melting-furnace; it is similar to the common brass-founder's furnace. Its internal dimensions are 42 inches square by 69 inches from the top to fire-bars, with a central pillar rising 15 inches above them to support the crucible.

C is the mould, as laid for a casting; and at

D is a crane, which commands A, B, and C; it also commands E the open cradle, seen in section (fig. 8), in which is placed the crucible full of fused metal previous to pouring.

F is an inclined causeway leading to the mouth of the annealing-oven, G. This oven was constructed with great care; it is circular, to ensure uniformity in the cooling of the enclosed speculum, and is strongly bound with a massive band of iron to prevent its cracking. Upon a thick foundation of rubble-work were laid several courses of brick, with a layer of sheet-iron interposed to prevent moisture rising from the ground, and on this were constructed the fireplace and flues for heating the bed of the oven. This bed is formed, first, of several courses of fire-bricks, on which are laid fire-tiles 12 inches square, previously ground separately to the required curve, and then set so as to form a continuous bed of the radius required for the under side of the speculum. The circular wall of the oven is 27 inches thick, and some details of its construction are shown in figs. 1 & 8. The system of flues below the bed, of which one or more can be readily stopped, if required, gives great facility for heating it equably; and the wall and arched roof of the oven (which was covered with 10 tons of sand) are heated by a mixed fire of coke and turf burning in the oven itself, the combustion being regulated by its independent air-hole, damper, and chimney. For the quick opening and closing of the mouth of the oven, a series of cast-iron stoppers were provided, coated internally with loam, and with removeable handles outside. These, when *in situ*, complete the circle of the wall, and give great facilities for opening and closing the oven when the speculum is introduced.

A single crucible, of cast iron, holds enough of metal for a casting; it was cast with the precautions indicated by Lord Rosse. Its internal dimensions are 25 inches diameter, 32 inches depth. The bottom is round, and its thickness is from 2 inches there, to $1\frac{1}{2}$ inch at top. It easily holds 30 cwt. of the alloy, 27 cwt. being required for a 4-feet speculum.

The speculum A was cast September 22, 1866. For eight previous days the annealing-oven had been heated up to a full red. The furnace was lighted the evening before, and the charging the crucible commenced at 2 A.M. At first the alloy was supplied slowly, for fear of cracking the cast-iron crucible, and the entire 27 cwt. was fused at 11 A.M. During the last hour of this period, the bed of hoops, which had been washed with two parts animal charcoal and six of plumbago mixed with ale, had been heated on the hearth A (fig. 1) till blue. The crucible was now lifted by the crane from the furnace, and transferred to the cradle E (fig. 1). The tackle of the crane was now shifted from the crucible to the cradle, and tilted till the metal reached its lip; the metal was once more poled and skimmed, and by rapidly turning the winches of the crane the pouring was effected. When the metal covered half the mould on one side, and at the other rose to the edge of the loam-ring at its lowest part, the mould was rapidly lowered to be hori-

zontal, the pouring continued without interruption, and the fluid covered the entire surface with an unbroken wave. The entire time was sixteen seconds. When sufficiently cool, the central core was removed, and the speculum, still on the bed of hoops, was drawn to the oven's mouth. This was effected by a powerful crab, placed outside the building, acting on a chain passing through the opposite side of the oven, and hooked to a bight of chain passed round the bed of hoops; the latter was now close to a step formed at the oven's mouth, and the bottom of the speculum was on a level with the oven's bed. The bight of chain was then raised to the loam-ring, and the bolts which fastened this to the bed of hoops being removed, the ring and the speculum were drawn into the oven off the bed.

The iron stoppers of the oven already described were set in their places; hot bricks were placed outside of them, and the whole luted up. All other apertures, including the entries to the chimneys, were carefully stopped. In doing this, a thermo-couple of platinum and iron wire was inserted, so that its joint was nearly over the centre of the speculum; and its elements were connected by copper wires with a galvanometer in Mr. Howard Grubb's office, where watch could be kept of the cooling. The initial deflection was 73, and in 23 days it showed that the temperature was nearly that of the atmosphere.

Speculum B was cast November 24 without any change, and both disks turned out exceedingly perfect; but it is little to have got a perfect disk of metal unless it can be figured and polished with equal perfection. For a long time it was believed that these processes could only be successfully done by the hand feeling the action; but Lord Rosse found that it could be performed as well, and with more certainty, by machinery. In this he was followed by Mr. Lassell and Mr. Warren De La Rue; and though their methods differ, the general principle is the same. The speculum revolves slowly on its axis, while the polisher traverses it more rapidly, describing a track which is some continuous curve, and crosses it in every possible direction.

Mr. Grubb's machine is remarkable for its simplicity, and the precision and smoothness of its action.

* Fig. 9 is an isometrical view of the grinding- and polishing-machine. A, A are strong A-shaped castings, connected by three collared stay-bolts and nuts, and having V-shaped bearings at top, in which turn the circular ends, or trunnions, of the hollow prism-shaped casting B. This casting is bored through its centre, and a circle on its upper side truly faced; to it is fitted a spindle with a large face-plate, whose under surface applies to the trued surface of the hollow beam for steadiness, and which serves to carry the speculum. The lower end of the spindle carries the wheel C, by which circular motion is given to the speculum, C being driven by the wheel and pulley D, by means of a belt driven from the shaft. To

* To avoid confusion the reference numbers are the same as in the Plates in Phil. Trans. 1869.

the framing are attached the brackets E, E, which carry the plate F F; the plate can be raised or lowered to suit different heights, and it carries the horizontal shaft G, which, by means of two pairs of bevel wheels of equal numbers, drives the two vertical shafts with their cranks H, H. These cranks are adjustable as to length. The rods (I, I) act conjointly upon the lower end of the vertical bar K, and this bar entering a central hole in the grinder (or polisher), produces the requisite horizontal movements in either process. It may be seen that by the adjustment of the strokes of the cranks H, and of the length of the rods I, a motion can be produced varying from nearly a straight line through a series of ovals to a circle of any extent, and either concentric with, or eccentric to, the underlying speculum.

The vertical bar K passes at its upper end through a hole in a bar attached to the roof of the building, and besides producing the horizontal motion mentioned, performs another important office. To the lower end of the bar attaches, by a cross key, a flanged socket which supports the triangular piece I, and at its upper end the bar is attached to a lever with adjustable weights, so that a pull upwards in the direction of the bar to any desired amount is provided, and thus any portion of the weight of the grinding- or polishing-tool in excess of that desired is relieved. The figure shows the details of the system of support as applied to the grinding-tool. The central piece L being upheld by the bar K, supports the three straight bars M, and these again support the six triangular pieces N. Thus any portion of the entire weight of the tool is supported from eighteen points.

The following arrangement was provided for the quick and convenient trial of the speculum while undergoing the process of polishing. To the hollow beam B is attached the apparatus O, P, Q, O being a massive toothed sector, P an endless screw working in the same, and Q a wheel and pinion with a hand-winch. This part of the apparatus serves either to retain the speculum in a horizontal position for polishing &c., or for quickly bringing it to the vertical, when by the opening of doors, purposely provided in the building, the speculum can be tested on a day object sufficiently distant; and again by a reverse motion of the winch the speculum is quickly restored to the horizontal position for continuing the process if required: this facility of testing the figure is of the highest importance to the ease and certainty of polishing; without it one would be working in the dark.

The first operation on the specula was to cut out, with a crown-saw and sand, the disks of metal 1½ inch thick which closed their central apertures. The front was then ground roughly to the approximate curve. The back was ground flat*, and the offset on the edge ground true. Then the speculum was placed on the supports, to be hereafter described, on which it was ultimately to remain, and the grinding of the front was completed.

The grinder was a disk of cast iron of the same diameter as the

* Better had it been spherical and concentric with the front.

speculum, and of the same curvature (convex of course). Its surface is divided into squares of about 3 inches, with half-inch spaces. It was at first charged with sand and water, and afterwards with very fine emery and water. The pressure in grinding was, on an average, 112 lbs., and the number of strokes of the machine 32 in a minute, and of considerable extent. The speculum revolves once for 14 strokes. The rough grinding occupied 650 hours, and the fine grinding 520 hours.

The polisher is built up of a great number of slips of fine deal laid in layers and crossed, each layer being firmly glued and nailed to the next. The face was turned to the requisite curvature, and strongly varnished. This is coated with pitch, adjusted to the requisite hardness by Mr. Nasmyth's test; this material was rolled into slips of uniform thickness, and cut by pressure into $\frac{1}{8}$ inch squares, while still soft, and these were applied to the surface of the polisher, by softening one side of them over a gas- or spirit-flame; the intervals between the squares were $\frac{5}{8}$ inch. It is needless to go into the details of polishing, which have been fully described by Lord Rosse and Mr. Lassell, beyond remarking that both the pressure and the lengths of the strokes are about $\frac{1}{5}$ of what are used in the grinding.

The small specula were polished on a similar machine.

As the large specula must be parabolic for the purposes of photography, the small ones must be specially figured to match them; but in this there is no difficulty.

One of them is of Lord Rosse's alloy, one of Mr. Kingsley's alloy, which differs from the other in containing $\frac{1}{4}$ of an equivalent of zinc. This undoubtedly gives a whiter image; and if it be as little liable to tarnish as Lord Rosse's, will be preferable.

The third is of silvered glass, not on Foucault's plan, but of a construction described originally by Mr. Grubb in 1857. It is an achromatized combination of crown and flint, the outer surface of which is of a curvature which prevents the formation of a false image. The hinder surface is coated with a thick deposit of silver. It is expected that this will reflect more light than either the metallic specula, or one of Foucault's silvered mirrors.

The uncertainty of the polishing process arises from the pitch not being of a proper hardness, and from the hygrometric state of the air. It must be always a very delicate operation, though Mr. Grubb seems to have reduced the difficulties to a minimum.

But a speculum, however perfect, cannot perform well unless it be supported behind and laterally, so as to avoid any irregular strain on it. For the back support, nothing has been found better than the system of equilibrated levers contrived by Mr. Grubb for the Armagh Cassegrain.

Figs. 12 & 13 show this arrangement. In fig. 12 A shows one of the primary levers, on the ends of which are supported on their centres of gravity the two secondary levers B, exhibited in the second and third of the diagram.

These again support six tertiary levers shown in the compartment C, of which the four triangular are triple, and the two straight are

double. These tertiary levers support the speculum by intervening balls, to avoid transverse friction.

Fig. 13, taken from a photograph, gives a general view of the levers. They are made of steel for strength and lightness; and by a happy device of Mr. Howard Grubb, the secondary levers, instead of being as usual placed between the primary and tertiary, are placed above the latter, nearly touching the under side of the speculum. The balls of the tertiary ones acting, when necessary, through holes in the secondary; the result of this is that the distance between the back of the speculum and the bottom of the box is only $3\frac{1}{2}$ inches.

Mr. Grubb's method of lateral support is shown in fig. 10; D is the section of the ring of wrought iron trued after being attached to the inner side of the speculum box.

E another ring of the same material, which fits with a slight looseness on D, and also on the ground edge of the speculum. When the telescope is inclined, the speculum pressing on the lower part of the ring E must bring its highest part into close contact with the ring D, while the lower part of E is removed out of contact with D, which virtually places the speculum under the same conditions as if it were held in a flexible band, of the form of the letter U, supported at its upper ends.

The tube consists of two parts, as shown by fig. 15; the lower, which is made of strong sheet iron, is connected below to the speculum box by three strong bolts. Between two of these is a slit, through which, by means of guides passing through two openings above it, a cover which fits closely to the top of the speculum-box can be introduced to protect the speculum when not in use. Then also the large eyepiece is replaced by a vessel containing quicklime to absorb moisture. The upper part of the tube is latticed work made of slips of steel, each crossing of which is secured by a rivet. Strong iron rings are rivetted to each end, and four strong diaphragms similarly attached at equal distances in its interior. As a proof of its stiffness, may be mentioned that a weight of 112 lbs. hung at its extremity caused only a deflection of $\frac{1}{200}$ of an inch.

The Finder has 4 inches aperture.

The equatorial which carries this huge telescope is not less remarkable. It will be seen in fig. 15 that the polar axis is inverted from its usual position, so that the declination-axis and the circles are near the ground.

The axes are exceedingly massive, but from the beautiful arrangement of their counterpoises, their motion is very easy. That shown at K relieves the upper pivot of the polar axis from its lateral friction. The same thing is done for the lower pivot by a sector shown, though imperfectly (fig. 15), below the circle R. This sector is carried by a sliding frame, which is forced up by a screw acting through steel springs, so as to relieve any desired portion of the lateral pressure, which is about six tons. The pressure on the lower pivot of the polar axis, which is about five tons, is relieved by the apparatus shown at L. The result of these arrangements is that the instru-

ment is turned round this axis by a force of 5 lbs. at a leverage of twenty feet.

The declination-axis has also its three sets of counterpoises. The weight of it, with the telescope and counterpoises, may be conceived as the resultant of three rectangular components,—one parallel to the polar axis, one parallel to the declination-axis, and one perpendicular to both. Of these the first counterpoise only appears in fig. 15, the others being concealed in the cube. But they are described with great minuteness in the paper.

As the size of the rollers is limited by the dimensions of the cube, they are not quite so effectual as the counterpoises of the polar axis; for it requires $12\frac{1}{2}$ lbs. acting at the leverage of 20 feet to turn the telescope round its axis. Still one man can raise the telescope from the horizon to the zenith in 20 seconds. In reversing it from one side of the pier to the other, two men are necessary for quick work, as it must be moved in P. D. as well as in A. They do it in 45 seconds.

The fixed and differential A circles are seen at R. They are 34 inches diameter, divided on an alloy of palladium and silver, to one minute; and the verniers read one second.

The P. D. circle P, of 30 inches diameter, divided on the same alloy, reads by its verniers to ten seconds. Besides the quick and slow movements of the two axes, in which there are several novelties, the clock for moving the instrument in A requires some notice.

Its position in the pier is shown (fig. 15) at Z, and its governor at fig. 31: *a* is a double fork-shaped frame of gun-metal, in which hang the two T-shaped pieces *b* and *b'* carrying the balls A and A', 5 inches diameter, of hollow brass filled with lead. These, when not in action, are retained near their working angle 45° by the piece *c*. Attached to the steel bars are small brackets, *d* and *d'*, carrying screws with divided heads, *e* and *e'*, the extremities of which terminate in small cups to contain pieces of hard leather. These, when the balls attaining their speed fly out, rub on the disk B, and thus prevent any increase of speed. The lower bearing of the governor's spindle is attached to a sliding-piece C, which is raised up by a mechanism, which moves an index in front of the clock-frame playing round a graduated arc, and kept in any desired position by a pin and a circle of holes. When the clock is adjusted to sidereal time, small differences, as in the case of planets, are corrected by the mechanism just mentioned.

For the moon, the change to mean lunar time is made by a set of differential wheels, which are brought into play in an instant, and the final adjustment to the moon's rate made as above.

The clock is connected by the shafts and gearing, *e* (fig. 15), with a screw working on the teeth of the clock sector D.

The sector, of gun metal, 5 feet radius, is fitted to turn accurately on a portion of the polar axis immediately above the hour-circles. Extraordinary precautions were taken in cutting the teeth of this sector, which are believed to be as exact as many dividing engines. The framing of this sector carries at 3 feet radius a tangent-screw

communicating with the $\mathcal{R}$ clamp by a nut of peculiar construction. When this clamp is free, the instrument can be moved in $\mathcal{R}$ by the quick motion G. When clamped, if the clock-work is in motion the telescope follows the object, but it can be moved independently by the tangent screw: *vide* fig. 22. (See the paper in Phil. Trans.)

Mr. Grubb had intended to supplement this clock-motion by a contrivance controlled by an ordinary seconds' pendulum, which was to add or subtract motive power, but this was not found necessary. The clock has great excess of power over any resistance likely to come into play; and it was found that doubling its actual driving-weight (200 lbs.) accelerated its rate by $\frac{1}{360}$.

The telescope has nine powers, ranging from 220 to 1000. It has also a micrometer, whose wires and bars are illuminated in a dark field by a peculiar apparatus. In this the light of a lateral lamp is received upon a concave speculum inclined at an angle of 45°, and placed so far in front of the wires that the rays of the lamp come to a focus on them. There is an aperture in the centre of this mirror of such size as to transmit to the eye all the light reflected by the large speculum. Now, in the Cassegrain it has been already mentioned that there is an eyestop so adjusted as to exclude all light but that which comes from the great speculum, therefore it is evident that no direct light from the mirror can reach the eye while the wires will be seen illuminated. The light of the lamp is modified by coloured shades to any degree of intensity.

The micrometer has powers from 300 to 600.

Photograph.—Some photograms of moon and stars taken with a temporary apparatus were considered by the Committee to be of such good promise that they directed a very complete one to be provided, similar to that used by Mr. W. De La Rue, with only such modifications as the great bulk of the telescope makes necessary.

The small speculum is, of course, removed before applying it. Fig. 35 is a plan, and fig. 36 half section, half elevation of it; A A A is the angle-iron forming the upper end of the telescope, to which is attached the steel tripod frame B B B, by the thumb-screws x, x, x. This tripod supports the pair of brass tubes e, f; the steel arms being connected with the outer tube e by the gun-metal rings C and D (fig. 36). The frame carrying the prepared plate is attached by a convenient arrangement to the upper end; D is worked by a lever and shaft, K, shown more fully in the next figures (figs. 37 and 38).

The adjustment for focus is made by the milled head g (fig. 36), working the rack and pinion h, while a scale of divisions, s, is used to register the exact position of the focus. Fig. 37 is a plan, and fig. 38 is an elevation of the exposing shutter. B, B, B (fig. 37) are, as before, the three arms of the tripod frame, C is the lower of the two gun-metal rings to which these are bolted. To this ring is attached, by the two mill-headed screws E, E, the gun-metal frame D (shown hatched in the figure) which carries the shaft c in a pair of bearings f, f, to which is attached the thin sheet brass shutter F; a is the shaft lettered K in fig. 36, which is acted on from below by the lever K (fig. 36); and a pair of parallel cords passing down the

side of the tube. This shaft, in turning through 90°, communicates a similar motion to the shaft *c*, which carries the shutter by the pair of cranks *b*, *b* and the connecting-rod *d*. As it is desirable to be able to open the shutter from either the right or left side to give a greater or less exposure to a particular side of the moon, this shutter is so arranged that, by taking out the two screws E, E, the frame D, which carries the shutter F, can be turned round 180° and fixed in that position; while the connecting-rod *d*, being disconnected by taking out the stud *x*, is thrown to the other side and attached to the spare crank *b'* at the other end of the shaft *c*.

At the same time the edge of the shutter can be adjusted parallel to the terminator of the moon by the rocking-piece G. The necessity, or at least the advantage of this arrangement, was shown in the experimental trials already mentioned; for it was found that to obtain tolerably uniform pictures of the whole moon many times more exposure was required for the parts near the terminator than for the bright edge. The apparatus includes a photographic micrometer similar to that described by Mr. W. De La Rue in his "Account of the Solar Eclipse of July 18, 1860," Phil. Trans. vol. clii. p. 373, for tabulating the photograms.

Spectroscope.—The spectroscope, with which the telescope is provided, differs from the ordinary construction only in being furnished with Mr. Grubb's compound prisms, which, with a considerable dispersion, have a small deviation. They are composed of Chance's extra dense flint, to which are cemented outer prisms of light crown glass. The instrument is of remarkable compactness and solidity.

The weight of the moving parts of this huge telescope is 18,170 lbs., of which the great speculum and its box and support are 3500.

THE END.

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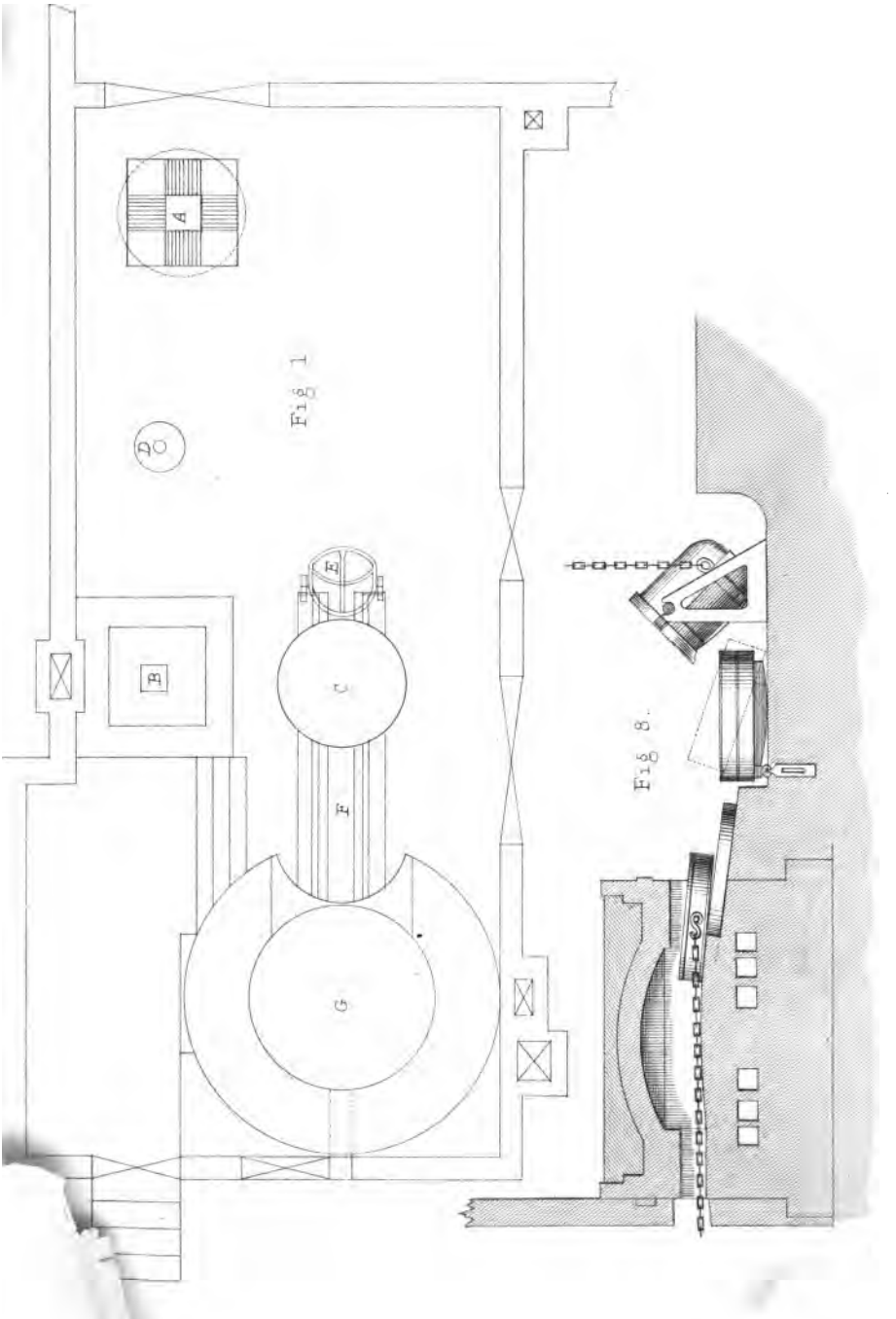
A
B
C
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E
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G
Fig 1
Fig 8.

Fig. 9

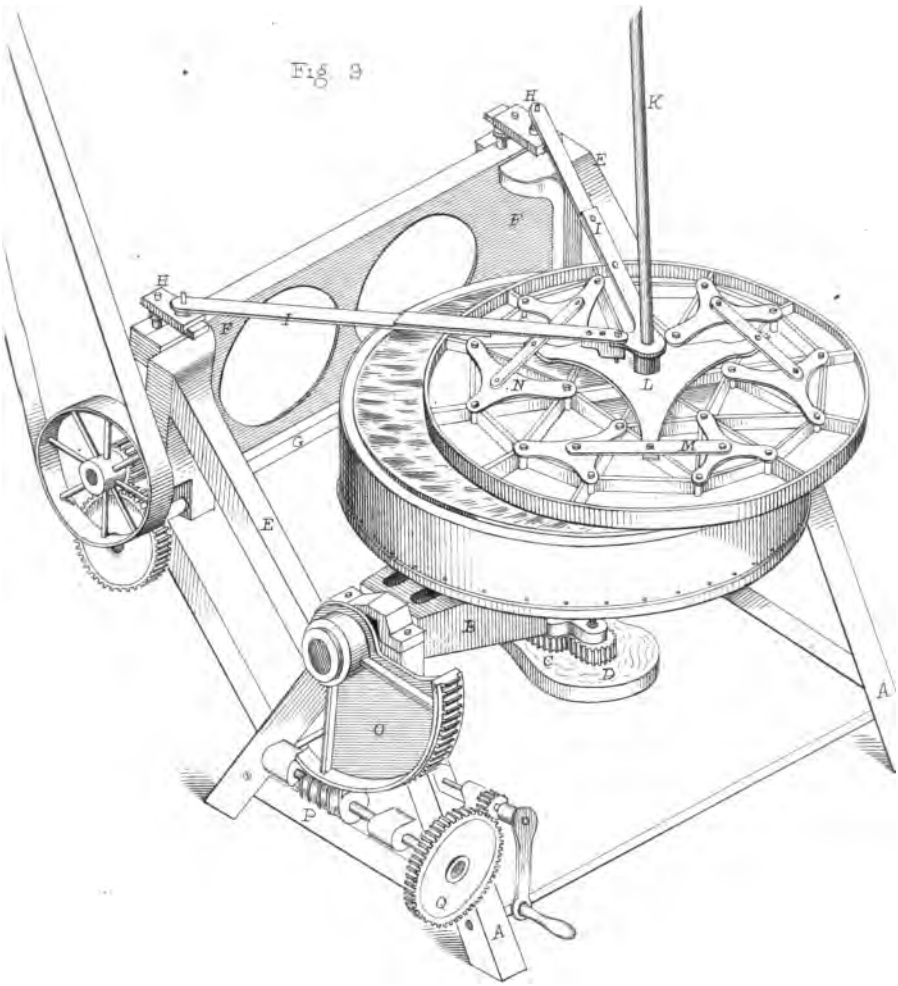

Fig. 10.

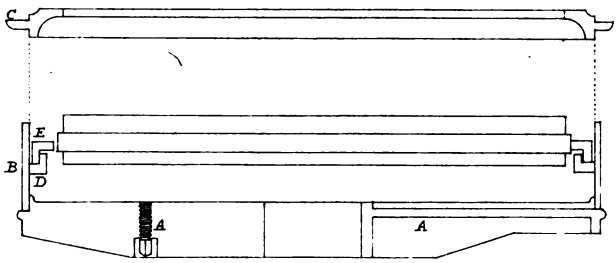

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Fig. 13.

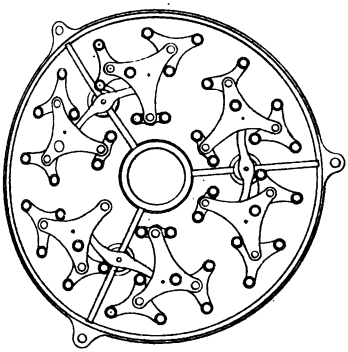
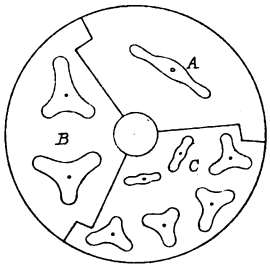


Fig. 12.



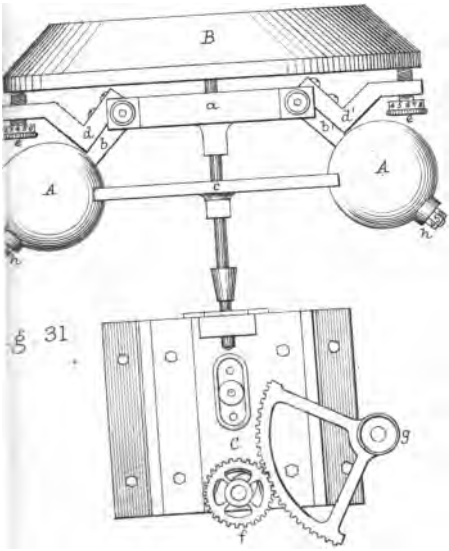


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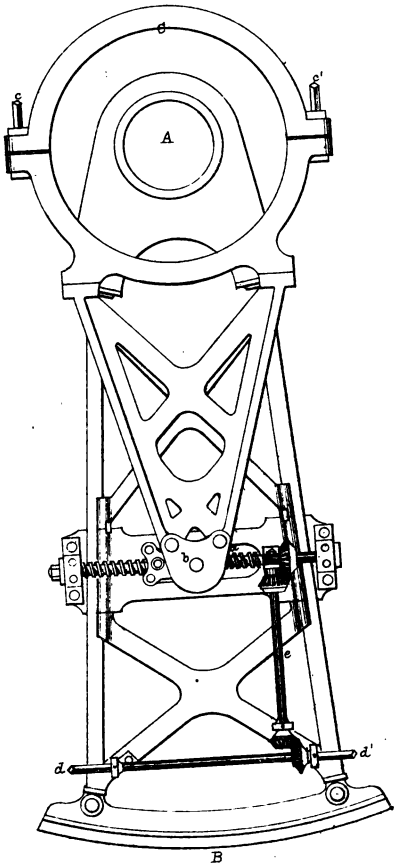
Fig. 22.



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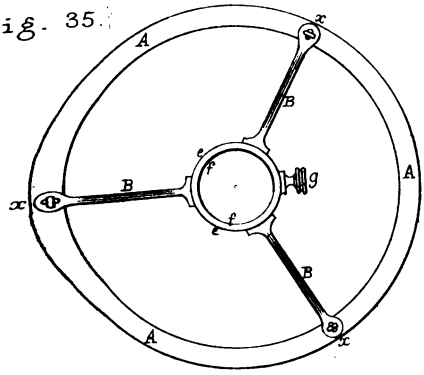

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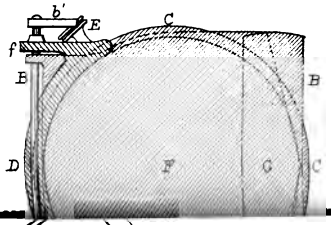
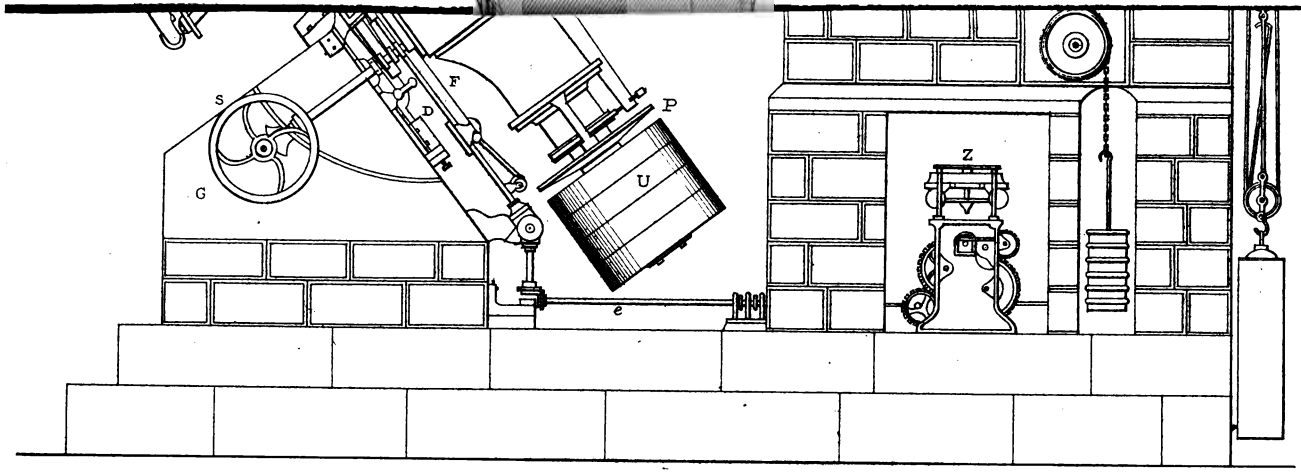


Fig. 37.



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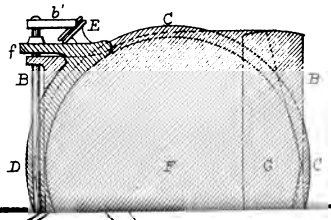
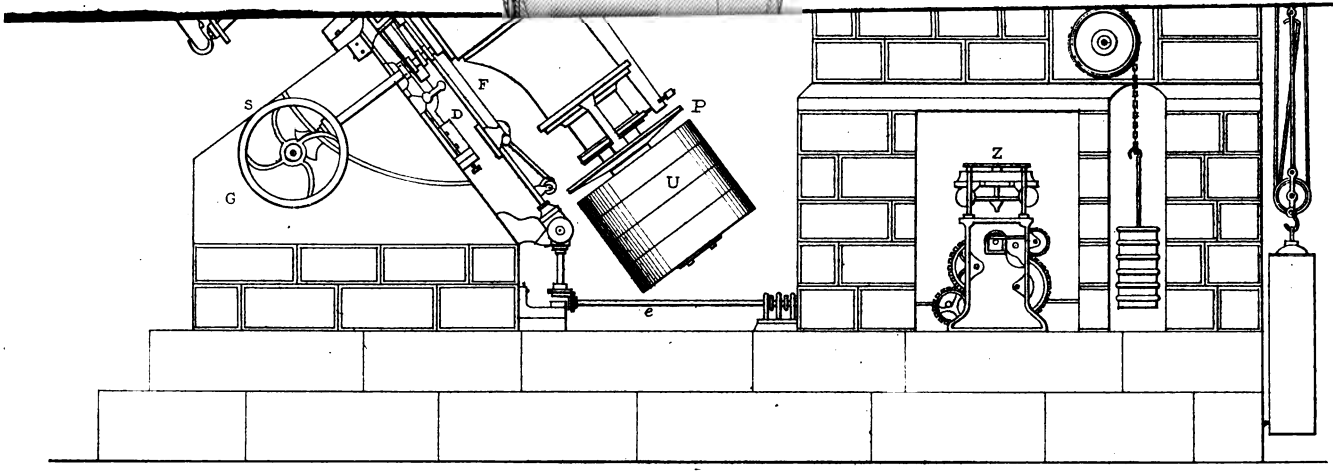


Fig. 37.



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